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COMPARATIVE PSYCHOLOGY MONOGRAPHS

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Self-Differentiation of the Basic Patterns of Coordination

PAUL WEISS

University of Chicago

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SELF-DIFFERENTIATION OF THE BASIC PATTERNS OF COORDINATION*

PAUL WEISS

Hull Zoological Laboratory, The University of Chicago

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THE PROBLEM OF COORDINATION

Introduction

Motor behavior is effected through the coordinated operation of the musculature. The problem of the ontogenetic origin of behavior, therefore, resolves itself essentially into the problem of the ontogeny of coordination.

Whether "coordination" is a *constitutional*, i.e. pre-functional, faculty of the central nervous system or an *acquired* property, gained by experience, has long been a matter of dispute. The

* The experimental investigations were aided by the Dr. Wallace C. and Clara A. Abbott Memorial Fund of the University of Chicago.

main objective of this paper is to present direct experimental proof that *the basic patterns of coordination arise by self-differentiation within the nerve centers, prior to, and irrespective of actual experience in their use*. In addition, experimental data will be reported indicating what coordination consists of, and how it is laid down in the centers. So specific and articulate has this experimental information been that we would forego the full benefit of its instructiveness if we were to report it in the general inarticulate language in which the problems of coordination are conventionally treated. Since the experiments have produced answers much more differentiated and detailed than any of the questions commonly being asked, we must prepare the ground for their presentation by reformulating the questions with greater precision. One cannot discuss "coordination" profitably so long as the term is kept on the abstract level; ill-defined, and non-committal in regard to concrete implications. We, therefore, shall try to dissect the general concept of "coordination." By breaking it down into more tangible issues, we make it tractable and give experimental analysis a chance to substitute knowledge for conjecture.

Thus, with a view to discontinuing the practice of speculating about "coordination" without a clear mental picture of just what it implies, we shall first review the facts and reformulate the problems, and only then proceed to present the experiments and, in their light, scrutinize the existing theories of coordination. The notable confusion about terms and facts in this field would justify a much more thorough reconsideration of the whole subject than can here be afforded. However, such an ambitious attempt had better be postponed until more concrete building stones for a good and sound theory of coordination have been gathered than are now available. It is only a few of these building stones that the present paper aims to contribute.

There is a striking disproportion between our knowledge of the physiological properties of nervous *elements* and our understanding of the operation of the nervous *system* as the coordinator of those elemental activities in the service of the organism. While the combined efforts of electro-physiological, histological, bio-

chemical and mathematical studies have produced a great wealth of data concerning the *elemental* activity of the neurone, our conception of the *systemic* activity of their organized totality has essentially remained pegged to the level to which it had been raised by *Sherrington's* classical work on the "Integrative Action of the Nervous System." We do not mean to imply that some progress has not been made here and there. Brain physiologists and neurologically-minded psychologists, in their efforts to interpret behavior in terms of the function of the nervous system, naturally had to focus on the system as such. From their studies, they were led to conclusions which partly supplemented, partly discredited, the synthetic conception of the nervous system to which preoccupation with the nervous elements had led. Yet, although they succeeded in pointing the direction toward a more adequate theory of central functions, the actual progress made thus far appears small when contrasted with the spectacular growth of our information about the nervous units during the same period. A mere comparison between the volume of attention currently paid to the issue of synaptic transmission—whether chemically or electrically mediated—on the one hand, and the almost complete neglect of the problem of how central transmission has come to be so discriminatory and selective as to lead to coordinated responses, rather than to unorganized convulsions, on the other, puts the situation into sharp relief. A restoration of sounder proportions should be attempted, of course, not by detracting from the current vigorous trend toward the isolated elements, but by reviving interest and revitalizing research in matters which concern the integrative aspects of the nervous system. If such course is to be followed with profit, it will pay, at the outset, to examine the possible reasons of its lag in the past.

A few explanations suggest themselves quite readily. The most obvious one is the infinitely greater difficulty and complexity of the task facing the student of the nervous system. This may be a challenge to inquisitive minds, but it certainly does not predispose the subject for mass attack by routine methods. So long as one clings to the study of elements, one is dealing with well-circumscribed units, a well-defined subject, presenting clear-

cut problems, and one can call on familiar and approved methods of analysis. As soon as one raises the eye from the unit to the whole system, the subject becomes fuzzy, the problems ill-descript, and the prospect of fruitful attack discouraging in its indefiniteness. This may explain why a considerable number of able experimental workers prefer to circle around the focal problems at a respectful distance rather than heading straight at them. It also explains why discussions of central nervous function operate so much more liberally with words than with facts; for it is remarkable how general the tendency is in this field to cover up factual ignorance by verbalisms. The average attitude is somewhat like this: the "whole" gets a large share of one's thought and talk, but the elements get all the benefit of one's actual work; here the problems seem to be so infinitely more tangible. *Adrian* (1932) has expressed this very plainly at the conclusion of his lectures on "the mechanism of nervous action." He says (p. 93): "The nervous system is built up of specialized cells whose reactions do not differ fundamentally from one another or from the reactions of the other kinds of excitable cell. They have a fairly simple mechanism when we treat them as individuals. Their behavior *in the mass*¹ may be quite another story, but this is for future work to decide." The search for that "other story" deserves encouragement.

However, a second point which has detracted from a vigorous pursuit of this search is not to be overlooked. This is the reiterated expression by highly competent students of the nervous system of their conviction that a thorough understanding of what is going on in the isolated peripheral nervous units, will eventually explain the operation of the centers, too. *Gasser* (1937), for instance, in his Harvey lecture on "the control of excitation in the nervous system," states this belief quite explicitly (p. 171): "Admittedly the nervous system can be understood only as it is operating as a whole, but it is equally true that an insight into its working can be gained *only*¹ by a detailed analysis of its parts. If the isolation of a part results in the sacrifice of some of its qualities, the loss is compensated for by the acquisition of a degree of sim-

¹ Not italicized in text.

plicity making the part more amenable to investigation. The organization of the nervous system is such that an understanding of the mode of activity of any part of the ganglionic apparatus would mean a long step forward, for all parts of the nervous system are fundamentally alike. We can, therefore, proceed, confident in the belief that when the parts are understood, they *can be added together into larger units*;² and that, as the addition takes place, the lost qualities will again emerge and be recognized. Bit by bit it should be possible in the end to build back to the elaborate patterns of activity which are characteristic of the intact organism." This stand seems justified if one accepts the premise proposed by *Keith Lucas* and quoted and amplified by *Gasser*, "that the phenomena taking place in the central nervous system could be explained without the assumption of any properties which could not be experimentally identified in peripheral nerve." "If the reactions in the central nervous system are to be explained on the basis of occurrences in peripheral nerves, . . . it is evident that the starting point of all discussions must be a thorough-going understanding of the physiology of nerve fibers" (p. 173).

At the same time, the opinion is growing that this precept of *Lucas* has not worked, and cannot work, in bringing us real understanding of the centers, in spite of numerous ingenious hypotheses designed to make it work. *Forbes*, for one, once an outstanding exponent of *Lucas*' scheme, expresses this change of opinion poignantly, when he declares (1936): "In developing these schemata no attempt was made to offer a final theory of the workings of the central nervous system, but merely to see whether research had yet brought to light any facts which were utterly incapable of explanation in terms of the phenomena of peripheral conduction. As newer information has come to light, the subsidiary hypotheses needed to explain the facts in terms of the nerve impulses alone have made it increasingly improbable that these working hypotheses would be adequate, until now they have become of little more than historical interest" (p. 164). Without arguing the issue any further, we merely present it as a symptom of the discouragement not uncommonly held out to

² Not italicized in text.

those who might have wanted to give the central nervous system an independent examination as a system in its own right.

Consequently, a large group of workers who did concern themselves primarily with the nervous system accepted the thesis of the "elementarians," that inasmuch as central activity can be conceived of as merely a proper linking together—"association"—of individual neurone activities, understanding of central activity can be pieced together from bits of knowledge about the elements. Thus the theory that "associations" form the basis of behavior, which is the psychological version of the theory of the "reflex" as the basis of central nervous action, played into the hands of the elementarians and greatly increased their prestige. Now, we still are not saying that they may not in the end turn out to have been right; but we do want to point out that the question as to whether there is more in the centers than there is to be found in the peripheral elements is a purely empirical question which cannot be solved on *a priori* grounds and which should not be prejudicated by recommendations for procedure which by their very nature preclude any but the anticipated answer. Nobody is likely to do prospecting in an area where he is constantly assured by experts that nothing worthwhile can be found; nor is he going to be encouraged by the repeated warning that whatever he is looking for has already been found—or, at least, will most certainly be found—by his neighbor. In this sense, the denial that the central nervous system presents problems *sui generis*, has undoubtedly been a potent deterrent from a vigorous attack on those problems.

It is not to be questioned that the attempt to identify in the central nervous system properties familiar from peripheral nerve elements has met with spectacular success. In fact, faith in the fundamental identity of both has been rewarded by discovery in the peripheral elements of properties which had formerly been known only to occur within the centers (see *Gasser*). However, by confining attention to those phenomena which the peripheral and central systems have in common, we plainly relegate the specifically central phenomena to continued obscurity; by "specifically central" we mean the ones that are not recoverable from peripheral

investigations. *Coordination* is a case in point, and this brings us to our immediate subject.

Even though we can interpret other central phenomena, such as reaction time, summation, rhythmicity, inhibition, irradiation, fatigue, etc., in terms of known properties of neurones, the specific *order* in which the units are brought into play so as to produce effects serviceable to the organism, is nowhere accounted for in this scheme. It is this order that is commonly referred to as "coordination," with implications that are not always clearly realized. Coordination means the *selective* activation of definite groups of units in such combinations that their united action will result in an organized peripheral effect that makes sense. But what principle is there in operation in the centers to make the appropriate selection? And in what terms is the choice being made? Here is a question aimed at a "specifically central" phenomenon, evidently of fundamental importance, and yet one that is hardly ever asked explicitly, and still more rarely answered in anything but the most general of terms. Really only a very few have taken the trouble of penetrating beneath the surface of the problem, and although, as we shall show below, none of their efforts have as yet yielded wholly acceptable results, they have at least emerged with some definite suggestions that can be put to test and serve as points of departure for further clarification. In addition to the few who have given the matter mature thought, practically every biologist and psychologist carries in his mind some sort of notion, specific or hazy, of the mechanism of coordination, which he has usually acquired unconsciously and by accident.

Disregarding their various shades, we can class these notions, both rational and instinctive ones, into three different groups: the *preformistic*, the *heuristic*, and the *systemic* theories. All three start from the fact of the transmissibility of excitation from one nerve element to another, and go on to explain why transmission in the normal nervous system does not occur indiscriminately, ending up in mass contraction of all muscles, but remains confined within certain channels, yielding an orderly and differentiated response. For the time being, we may ignore the fact

that by stamping the problem as purely one of controlled transmission we subject our search to an unwarranted limitation from the very start. Inasmuch as most speculations on coordination have tacitly accepted this limitation, they do not essentially differ on that point. From here on, however, they diverge.

The theories of coordination

The preformistic-structural concept

The first and most popular interpretation of coordination is the reference to stereotyped inherited anatomical neurone connections in the centers. It is based on the study of reflexes, the observation that in many simple reflexes there is a fairly definite and constant relation between the point of stimulation and the nature of the response ("reflex-arc"), and the assumption that the chain of events leading from stimulus to response is anatomically preformed in a chain of neurones leading from the sense organ through the centers to the effector. The biological adequacy of the response, according to this concept, is a result of the correct construction of the anatomical apparatus, that is, among other things, of the suitable distribution of the peripheral and intra-central neurones, suitable arrangement of the central switches among neurones, suitable arrangement of the muscles on the skeleton and suitable form of the joints; "suitable" in the sense of making the whole response come out as of service to the organism. In other words, the body has its coordination built in.

In designating this concept as "preformistic," we do not use the term "preformation" in the sense in which it is used in embryology. There it refers to the existence of organized patterns in the egg prior to the onset of *development*, while in the present connection it merely implies the presence of definitely organized innervation patterns in the centers prior to the onset of their actual *operation*. Developmentally speaking, these patterns have, of course, been differentiated according to the same principle of progressive (epigenetic) determination which dominates embryonic development in general (*P. Weiss, 1939*). Only in functional regards may we call them "self-differentiated," that is, differentiated in their essential characteristics independently of the actual intervention of function.

Movements with only one degree of freedom give an excellent illustration of this kind of anatomically preformed coordination. All a clam can do with its shell, is to open and close it, the contraction of the adductor muscle effecting the closure, and the elastic ligament over the hinge effecting the opening when the muscle relaxes. The whole performance is rigidly determined by the construction of valves and hinge. The situation would not be fundamentally different, if the opening of the hinge were effected by another muscle, instead of an elastic ligament, as is the case in bivalve brachiopods. In this case, the "suitability" of the situation implies, in addition to the hinging of the joint, the insertion of the two muscles on opposite sides of the joint (mechanically antagonistic action) and the ability of the nervous system to contract one muscle while the other relaxes (reciprocal innervation). If one admits the possibility that, for instance, tactile receptors are connected with the shutting muscle, and chemoreceptors with the opening muscle, the differential response of the animal—closing after mechanical disturbance and opening in the presence of food—would be accounted for on purely anatomical grounds.

Cases have been reported in which the anatomical predisposition to produce a given peripheral effect is even visibly expressed by the nervous elements so predisposed. The giant nerve fibers of the earthworm are the mediators of certain fast responses only, while other fiber systems serve slower reactions of different pattern (see *Prosser*, 1934). Making use of the fact that thicker fibers conduct faster (*Erlanger* and *Gasser*, 1935), the anatomy of these animals has provided for a system of central superhighways for undelayed through traffic. Still more spectacular is the case of the innervation of the mantle of the squid. The giant nerve fibers to the mantle muscles, which radiate from an anterior ganglion, increase both in length and thickness in anteroposterior order (*J. Z. Young*, 1938). In view of the proportionality between thickness and conduction speed, this provision enables a simultaneous discharge from the ganglion to arrive at all muscles at approximately the same time, producing a powerful synchronized over-all contraction in spite of the graded lengths of the supplying nerves (*Pumphrey* and *Young*, 1938). But for this anatomical

provision, the result could be achieved only by having the impulses go off staggered in definite regular intervals, beginning with the longest fibers. Here, too, coordination is efficiently preformed in the anatomical structure of the nervous system. Similarly, the fast and slow reactions of the claws of crustaceans are mediated by different sets of nerve fibers, either set being predisposed for its function by appropriate constitutional properties (*Wiersma and van Harreveld, 1938*).

There can be no doubt, therefore, about the existence of built-in coordination. The only question is whether these observations can be generalized and made the basis for a theory of *all* coordination. In point of fact, they have been generalized without much opposition. For instance, the flexing and bending of a knee joint has been viewed much in the same light as the opening and closing of the valves in a brachiopod. However, we must reiterate that the brachiopod exemplifies a system with only a single degree of freedom of motion. The only two movements compatible with the rigid mechanical limitations occur both in the same plane, and the muscles merely determine whether the joint will move one way or the other. It is after this sort of model that the anatomical theory of reflex action has been fashioned, and one recognizes immediately the close resemblance with the conventional description of the stepping movements of a vertebrate limb. The main action of the joints is represented as occurring in one plane, with two groups of antagonistic muscles producing excursions of opposite sign, which, depending on whether they reduce or increase the angle of the joint, are designated as "flexion" and "extension." To represent a limb movement in this manner involves a deliberate abstraction, in that we confine our attention to those joints which, in crude approximation at least, can be considered to conform to the model, that is, to true hinge joints (fig. 1, *a*). Under these conditions, simplified to the extreme, peripheral coordination would appear simply as the alternating contraction of two antagonistic muscle groups, and the central basis of that coordination might pass for merely an oscillation between the excitation of a "flexor" and that of an "extensor" center, each attended by in-

hibition of the other ("reciprocal" innervation).³ An underlying anatomical neurone set-up is conceivable, and several such schemes have been promoted in the past. They have become so firmly ingrained in our thinking that most textbooks deal with them as realities.

This is no place to evaluate the merits of the various anatomical switchboard concepts which have been advanced in explanation of simple type reflexes, and, in further consequence, of coordinated behavior in general. But it will be well to keep in mind what

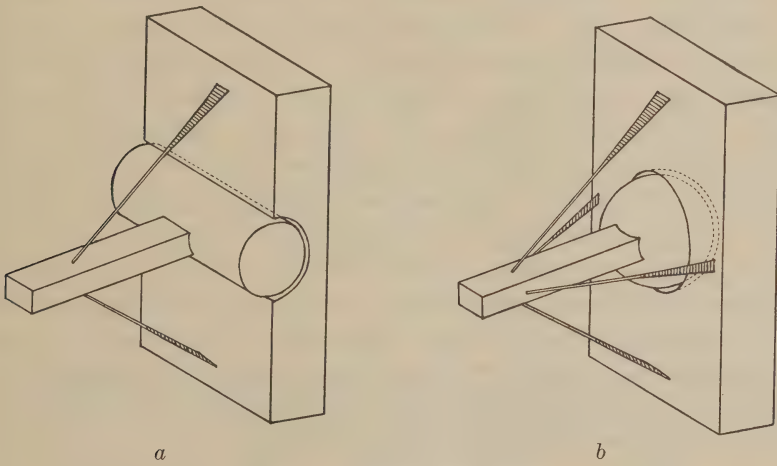


FIG. 1. MODELS OF JOINTS

a, hinge joint with single degree of freedom; moved by a pair of antagonistic muscles; *b*, ball joint with free rotational motion, operated by four muscles.

Herrick, that most judicious student of the anatomy of the central nervous system, had to say in this connection (1930, p. 645): "No complication of separate and insulated reflex arcs, each of which is conceived as giving a one-to-one relation between stimulus and response, and no interconnection of such arcs by elaborate switchboard devices can conceivably yield the type of behavior which we actually find in higher vertebrates. . . . These facts are

³ It is irrelevant in the present context whether this oscillation is regarded as due to alternating stretch-reflexes (*Sherrington*) or to autonomous rhythms of the centers (*Graham Brown*, 1914).

regarded as incompatible with the traditional dogmas of reflex physiology, with its precisely localized and well-insulated reflex arcs and centers of reflex adjustment. . . . The mechanisms of traditional reflexology seem hopelessly inadequate." However, even if the concept of a rigid anatomical neurone linkage were adequate to explain the single pendulum-like action of a hinge joint, it must utterly fail as a basis of coordination in any more general sense. For most joints are constructed so as to allow of more than one degree of freedom and, therefore, require more than two sets of muscles, inserting and acting in different planes.

A ball joint, such as the shoulder joint, can be moved in any plane laid through its center (fig. 1, *b*). The direction in which it actually moves at any given moment, is determined by the resultant of muscular tensions acting from all sides. Depending on the combination of active muscles and the relative strength of their contraction, an infinite variety of positions can be assumed. Variety of movement is thus made possible by varying the combination of muscles called into action. While it is easy to separate the muscles of a hinge joint sharply into agonists and antagonists, such classification is no longer applicable to a ball joint. Any two muscles may facultatively operate as agonists or antagonists. The following example, which refers to a specific case dealt with later in this paper, will help to make this clear.

Let us consider the shoulder joint of a tetrapod. Ignoring the finer details of the distribution of its muscles, we recognize four main groups converging upon the humerus from four different directions, schematically along the four edges of an imaginary pyramid (fig. 1*b*, 2). Acting individually, these muscles—listed in counterclockwise order—would pull the humerus upward, forward, downward, and backward, respectively, and they may be designated accordingly as elevator (El), abductor (Ab), depressor (De), and adductor (Ad). Through their graded contraction in proper combinations the humerus can be made to describe a full circle, eight representative stages of which are reproduced in figure 2. To bring the humerus into any of these eight positions, the following muscle combinations must enter into action (table 1).

According to the table, any one muscle may be engaged either

in phase or out of phase with any other muscle. The diagram (fig. 3) expresses for each pair of muscles the phases when they act synergically (convergent arrows) and antagonistically (di-

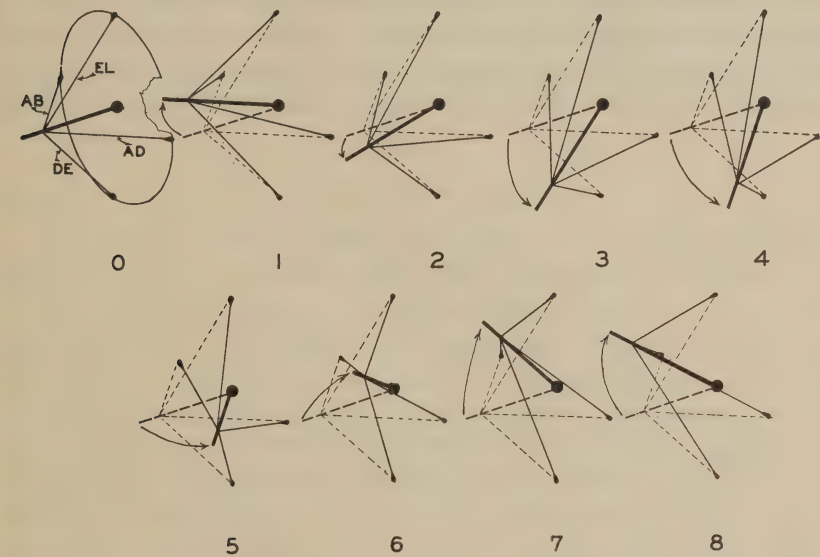


FIG. 2. EIGHT POSITIONS OF THE BALL JOINT OF FIGURE 1B, ASSUMED THROUGH THE CONTRACTION OF ITS MUSCLES IN THE COMBINATIONS LISTED IN TABLE 1

The posture in which all muscles are evenly contracted is pictured in phase 0; it is indicated throughout the following phases by dotted lines, the arrows showing the direction of the excursions.

TABLE 1

PHASE	MUSCLES IN ACTION	RESULTING MOVEMENT
1	El, Ab, De	Craniad
2	Ab, De	Cranio-ventrad
3	Ab, De, Ad	Ventrad
4	De, Ad	Ventro-caudad
5	El, De, Ad	Caudad
6	El, Ad	Caudo-dorsad
7	El, Ab, Ad	Dorsad
8	El, Ab	Dorso-craniad

vergent arrows). Thus, in contrast to the hinge joint with its single direction of motion, the ball joint presents the centers with a problem of multiple choice: the grouping of the mucleus changes

with the movement to be effected. The problem of coordination, therefore, is no longer simply one of alternative innervation of one out of two muscle groups ("flexors" and "extensors"), but it involves the selection of a definite combination of muscles out of a large number of possible combinations. If we add that a change in the intensity of the contraction of any one muscle necessarily changes the direction of the resulting movement, we realize that

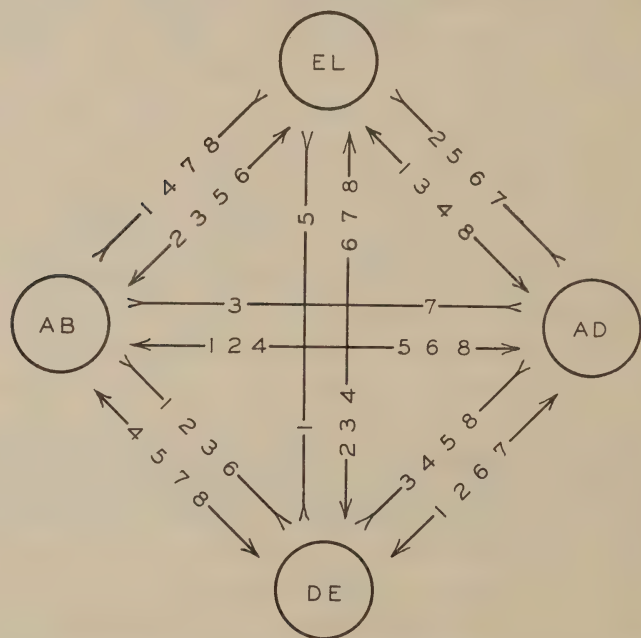


FIG. 3. DIAGRAM ILLUSTRATING THE CHANGING AGONISTIC ($\rightarrow\leftarrow$) OR ANTAGONISTIC (\leftrightarrow) RELATIONS BETWEEN ANY TWO MUSCLES IN EXECUTING THE EIGHT MOVEMENTS OF FIGURE 2

none of the schemata developed for hinge joints are applicable to this more general case, and, particularly, that this type of coordination must defeat any interpretation in terms of monotonous central connections. Here the problem of coordination presents itself in its full meaning: *What determines the choice of muscles to be engaged in a given movement, and how is this selective activation being put into effect?* Viewing the problem in the light of the ball joint, sets it into the right perspective, while the over-

emphasis of the flexor-extensor pendulum essentially misses the problem and thus helps to side-track rather than to solve it. We shall return to this subject later in the paper.

From a correct appreciation of the implications of the problem, several theories have arisen, striving to replace, or, at least, supplement, the thesis of pure structural preformation of coordination. We may call them the *heuristic* and the *systemic* theories.

The heuristic concept

Any theory which submits that the common appropriateness for the body of motor effects has been developed ontogenetically under the molding action of practice and experience, may be called *heuristic*. The animal is thought to be capable of activating each one of its numerous motor effectors independently. Through repeated trials with constantly changing combinations it is supposed to produce a kaleidoscopic variety of peripheral effects, of which some are successful from the standpoint of the body and its needs, some are failures, and some are indifferent. Combinations leading to useful effects are somehow preserved in the central organization and fixed and improved by repetition, while those leading to useless or adverse effects are eliminated. According to this view, patterns of coordination arise through the accumulation of nervous "associations" which have proved their usefulness for the body. The essential point is that the *effectiveness* of a response is thought to confer selective value, and hence stability, upon the originally wholly tentative grouping of the muscles through which it is brought about. Whether the trials in this "trial-and-error" procedure are entirely random, or whether they show some method and direction; whether the drive to move is produced within the organism or furnished by external stimulation; whether the "associations" are to be viewed as nerve fiber connections, or whether—following behavioristic maxim—one had better refrain from such attempts at visualization; all these are relatively minor matters compared with the basic tenet in which all heuristic theories agree: that the central nervous system is a plastic mold upon which experience gained in actual performance

gradually inscribes the patterns of coordinated behavior, with the adequacy of the effect for the organism as a whole serving as the standard of rating. This concept has been advanced for the lowest (*Jennings*, 1931) as well as for the highest forms of animals (*Pavlov*, 1927), and it has been variously applied to the development of nervous coordination from the highest cortical acts down to the most elementary motor functions.

The systemic concept

The systemic theories of coordination have in common with the heuristic theories the assumption of practically unlimited *plasticity* of the nervous system. However, instead of letting coordination become built up bit by bit through trial and error methods, they concede to the nervous system a *primordial dynamic ability* to respond to any change in the external stimulus situation by a total response of maximum adequacy for the organism as a whole. According to this view, entirely novel stimulus situations, neither provided for in the organization of the animal nor previously experienced, can be met by a primary response of great suitability. This view, shared by many Gestalt psychologists, has been particularly elaborated by *Bethe* (1931) and *Goldstein* (1939). The contrast between the heuristic and systemic concept can perhaps be expressed as follows: according to the former, partial reactions (elementary senso-motor responses) of no definite directiveness are variously recombined until they finally compose a chain the resultant direction of which has affirmed its value for the organism; whereas, according to the latter, resourceful dynamics of the central nervous system lay down the general direction of the total response as a sort of frame through which the partial reactions necessary for its execution are forced in channels consonant with the general "intention." In this view, the drive toward a certain real or visualized goal would directly produce the proper muscular innervation necessary to attain that goal.

Summary

Reduced to the terms of the preformistic-anatomical (1), heuristic (2), and systemic (3) theory, respectively, the coordinated

advance of an organism toward a desirable goal, and its coordinated retreat from a harmful situation, could be expressed as follows:

(1) Beneficial and nocuous stimuli enter through different adequate receptors, activate each a system of separate pre-arranged lines which, in turn, engage a pre-arranged selection of muscles in a pre-arranged time order, the combined action of which then becomes manifest as a motion of advance or withdrawal. The appropriateness of the response is based on the appropriateness of the inherited pre-arrangements; the individual itself deserves no credit for it.

(2) Either kind of stimulus evokes ubiquitous random reactions, including excursions of limbs and trunk of continuously varying patterns, which are tried and discarded and repeated and altered, until eventually the correct composition and sequence is discovered; the animal is to be given credit for its resourcefulness in producing ever changing assortments of undirected responses, as well as for its faculty to choose and retain those that prove to lie in the right direction.

(3) Any stimulus produces a general response the character of which is directly determined by the constellation of the external field of stimuli and the internal state of the centers, resulting in a primarily directed movement; the centers get credit for their ingenuity.

To reduce the various theories of coordination to such simple formulae, admittedly involves a great deal of abstraction and oversimplification. Moreover, many authors, in discussing these matters, have failed to take an explicit stand, which puts it up to the interpreter to extrapolate their basic beliefs from casual remarks; which is a doubtful task. Again, some have taken compromise attitudes, contaminating one theory with admixtures from another, which makes a strict classification of their views impossible. Hence, no more than practical significance should be attached to the attempt of the preceding pages to group all existing theories of coordination into the three outlined categories. The justification for that attempt lies merely in that it serves to crystallize the issues. It puts into specific form the premises and implications of the various current theories and

thus prepares them for the experimental tests to which they must be subjected for verification.

THE EXPERIMENTAL STUDY OF COORDINATION

The central hierarchy

To decide between the performistic and the heuristic concept of coordination, is an empirical problem. Here is the alternative which the experimental investigator faces: If coordination is preformed in self-differentiated central impulse patterns, which yield adequate peripheral effects only by virtue of what may be called evolutionary precedent and in the individual case amounts to predesign, they should prove stable and conservative even if experimentally prevented from producing appropriate functional effects. If, on the other hand, functional effectiveness is all that counts in shaping the patterns of coordination, one should expect any experimental reduction of that effectiveness to be followed by corrective modifications of the impulse patterns—evidence of plasticity and of lack of intrinsic organization.

It is evident that the decision cannot be reached by even the most intimate study of the *normal* organism, with its inherited stereotypism of central structures, peripheral structures and nerve connections. For this stereotypism means that the same standard central patterns and the same standard effects appear always in conjunction. A crucial experiment, therefore, must aim at disrupting the monotony of central-peripheral correspondence. It must upset either the discharge pattern of the centers or the play of muscles or the distribution of nerve connections in such a manner as to make the established central impulse patterns yield incongruous effects for the body. If, thereafter, the body recovers more efficient use of the affected part—either instantaneously by systemic reaction, or gradually by heuristic procedure—, the systemic or heuristic theories would score. If, on the other hand, corrective changes fail to occur and the nervous system continues to operate the part according to the old standard scheme of innervation now rendered inadequate, this would be incontestable proof of the *preformation* of coordination in form of definite central impulse patterns which do or do not produce

appropriate effects, depending on whether the effector system for whose operation they are predesigned is intact or disarranged.

All this seems so plain that one might expect the issue to have long been settled one way or the other. In fact, the indicated experimental course has been followed by some authors in the past. If, nevertheless, there is, even at this date, a basic lack of agreement, this suggests that either the experimental results or their interpretations have remained inconclusive. The reasons for this failure will become increasingly clear in the course of this paper. Two of them may be specifically mentioned here: preoccupation with higher mammals, particularly man; and injudicious generalization of concepts of "learning" or "conditioning."

The substitution of a healthy muscle for a paralyzed one is standard practice among orthopedic surgeons. The replacing muscle is sutured to the tendon stump of the incapacitated muscle and thus the mechanical part played by the lost muscle is taken over by the substitute. To be physiologically effective, the transposed muscle must, of course, be operated according to a new time schedule. Its former functional associations must be dissolved and replaced by new ones in accordance with its new function. To be sure, tendon transposition as such, even without re-timing of muscle actions, produces some degree of improvement, simply owing to the restoration of a more normal balance of tensions around the joint. For the shift of a muscle from the vigorous to the frail side not only reduces the bulk of muscle left on the intact side, but also cancels part of the remaining muscle power of that side by the opposing action of the shifted portion contracting simultaneously. Further postoperative adjustments are brought about by compensatory changes in the strength with which other, unimpaired, muscle groups are being engaged, still according to the original time pattern.

However, there is incontrovertible evidence to show that the gradual restoration of relatively efficient limb coordination in patients with translocated tendon insertions is not merely due to changes of the kind just mentioned, but involves an actual modification of the original time pattern of innervation, the transposed muscle assuming functionally—i.e., with regard to its phase of

innervation relative to other muscles—the place formerly held by the muscle which it has replaced mechanically. While there seem to be limitations to this adaptive change of coordination (*Scherb*, 1938), there is consensus of opinion that some “re-education” of the play of muscles can be attained by proper training in all human beings.

By sheer extrapolation it was then conjectured that what is true of man, would likewise hold for other animals. A broad experimental foundation of this assumption seems to have been neither sought nor offered. Some pertinent experiments were made on the coordination of eye movements after muscle translocation in mammals (*Marina*, 1912), but the results have remained controversial (*Dusser de Barenne and de Kleyn*, 1929; *Olmsted, Margutti and Yanagisana*, 1936). An isolated report of reorganization of limb coordination in the frog (*Manigk*, 1934) was shown to have arisen from a faulty interpretation of the underlying experiments (*Taylor*, 1936). But in spite of this lack of convincing experimental proof, the view seems to prevail that the locomotor apparatus of an animal can undergo essentially the same kind of adjustive “re-education” which has been demonstrated in man.

Critical experiments on amphibians, however, have contradicted this view decisively. As will be reported below in greater detail, these animals show no trace of re-adjustment of muscle coordination under comparable circumstances. To avoid misunderstandings, it may be added that while the basic coordination mechanisms through which all locomotor acts must be executed are in themselves quite unmodifiable in amphibians, the total behavior of these animals can be somewhat modified by training. They can learn to advance or retreat on different occasions, but they cannot learn to change their manner of walking or retreating. Similarly, recent experiments on the rat have definitely shown that the time pattern of coordination of the hind limb muscles of these animals, too, is rigidly fixed and remains incorrigible even after the crossing of antagonistic muscles resulting in permanent reversal of movements (*Sperry*, 1940). There may be a trace of re-adjustment after transposition of muscles in the *fore*

limb of the rat (*Sperry*, see below), and as we extend the examination to higher and higher mammals, we may expect to find a growing faculty for such corrective measures of the nervous system. The essential point, however, remains that this faculty is a very late evolutionary acquisition of the central nervous system, practically still absent in as high an animal as the rat, and consequently entirely unfit as a model of the principle of coordination in general. (See p. 80.)

We must once and for all renounce the idea that the type of muscular control with which we are most familiar, namely, our own, or at least that part of it of which we are consciously aware, represents the fundamental type of vertebrate coordination. Man can learn to engage individual muscles independently, but most animals cannot. This is why the anthropocentric approach to the general problem of coordination is misleading and has failed to produce results of general applicability whenever attempted.

The issue has often been further obscured by ill-defined and unverified generalizations of the concept of "learning." "Learning," that is, an adaptive modification of behavior in response to recurrent stimulus situations, has been demonstrated to occur, at least in traces, in most branches of the animal kingdom, from the lowest forms up. However, the strict constitutional limitations of this learning ability do not seem to be generally realized, or if so, have certainly not received due emphasis. There is agreement that the total motor performance of an animal can be modified by experience, but since the total performance is an integrated act, involving shifting combinations of partial performances of more elementary character, it remains to be demonstrated whether the modification concerns those elementary acts—the building blocks of behavior, as it were—as such, or merely their combination into more complex actions on a higher level. The mere assertion that the response mechanisms of the animal as a whole are not absolutely rigid but provide for some degree of adaptation, does not reveal whether this plasticity extends to all parts of the behavioral mechanism alike or is a privilege of certain components only, and if so, of which. Adap-

tive behavior presupposes functional reorganization somewhere; but where? Is the whole nervous system one vast pool of equivalent elements whose functional relations can be infinitely varied by experience, or is adaptability confined to some of its divisions or some of its functions only, while the rest are immutable?

The question is no longer whether learning is a common faculty or not. The answer to this has become a matter of course. What we need, is to know precisely what functions are amenable to change and what others are not; further, what functional elements, or groupings of elements, remain constant and unmodifiable even as the behavior pattern of which they form integral parts changes. Behavior results from the activities of a hierarchy of functional levels, each of which may or may not be adaptable. Plasticity on any one level neither implies nor precludes plasticity on any other level; the only means to test their capacities is by way of experiments.

Let us briefly illustrate the various levels.⁴ Confining ourselves to metazoans possessing a differentiated nervous system, we may dismiss subcellular entities and start right at the cellular level. There we find as the lowest recognizable elements to which a measure of functional stability may be conceded, the individual *motor units*, the term signifying, according to *Sherrington*, a motor neurone with its attached muscle fibers (level 1).

The orderly contraction of a whole *muscle* is the result of collective action of its constituent motor units, and the characteristics as well as the grading of the resultant contractile effect depend largely on the proportion of active to inactive units, on their rate of alternation, on the time relations of their activation (synchronization or temporal dispersion), on the frequencies of their discharges, and several other factors. In other words, in order that a muscle may function properly, its motor units must be definitely under common control. What happens when motor units act without control and at random, is impressively demonstrated by the functional inefficiency of a muscle in the state of

⁴ This list is more pertinent than one published on an earlier occasion (*P. Weiss*, 1925).

pathological fibrillation. The level of integration of the motor units of a given muscle may be designated as level 2.

Next, we must remember that every *joint* or other movable part (e.g., eyeball) is operated not by one, but by several muscles. The relative strength and timing of their contractions determine the direction and speed of the movement and the duration and stability of the resulting position. Thus the simple muscular actions of level 2 are integrated into orderly functions of muscular complexes relating to a single joint (level 3).

Again, in order to obtain an efficient movement or to maintain a definite posture of a segmented structure, such as a limb or the spine, containing *several joints*, the activities of the various muscle groups of level 3 must be finely correlated among one another, a task which gains in complexity in those cases in which a single muscle spans two joints, moving either one or the other or both, depending on the degree to which it is opposed at the time by other muscles. Thus, limb movement requires a higher level of integration (level 4) than does single joint action.

On the next higher level (level 5), we find movements of the various locomotor organs (limbs, trunk segments, tail) combined in orderly fashion so as to yield a definite act of the entire *locomotor system*, such as ambulatory progression and regression, jumping, swimming, or the like. The integration of widely separated muscle groups in the act of breathing (laryngeal, intercostal, abdominal, diaphragmal muscles) is obviously of the same order.

Finally, on the highest level common to all animals (level 6), the various motor acts are put into the service of the *animal as a whole* under the control of the sensory apparatus, whose reports are evaluated by the centers in accordance with the external stimulus situation, the internal state of the animal, the inherited response mechanisms of the centers, and such modifications of the latter as past experience may have brought about. It is in their bearing on this level that all motor acts of the lower levels gain *biological significance*. Viewed from this level, "progression" becomes an instrument of "preying," "regression" of "escaping," "eye movements" of "orientation in space," and so forth.

For abbreviated reference to the various levels, we shall use the following symbols:

Level 1, i.e., level of the Neurone.....	N
Level 2, i.e., level of the Muscle.....	M
Level 3, i.e., level of the muscle Group.....	G
Level 4, i.e., level of the Organ.....	O
Level 5, i.e., level of the organ System.....	S
Level 6, i.e., level of the organism as a Whole.....	W

Now, after this survey, let us repeat the question: On which one of these levels does "learning" occur? Possibly on all of them? Or on the highest (W) only? Or the lowest (N) only?

To those who are either unaware of the hierarchical principle of nervous function or may think to have grounds for denying it, these questions must seem utterly senseless. For them there exists no central organization on a level higher than that of the neurone, and as they would describe all behavior merely in terms of connections among individual neurones, so they would naturally be disinclined to conceive of "learning" otherwise than as of a free rearrangement of individual neurone connections. In the terms of our question this amounts to asserting that "learning" occurs on the level N, and exclusively there. There is no room in this concept for stability and unmodifiability of functions of lower order in an organism whose behavior has been demonstrated to be adaptable. It is either plasticity all the way, or rigidity all the way. "Learning," according to this view, proceeds by tentatively engaging, disengaging, and re-engaging independent efferent neurones in varying constellations, letting the biological value of the results for the organism decide which ones of the tried combinations are to be preserved, and which to be discarded. If this were true, the problem for an animal would be the same, whether it faces the necessity to substitute one established pattern of locomotion for another established pattern (e.g., hopping for running; see *Bethe and Fischer*, 1931), or to make modifications *within* a standard pattern (e.g., longer strides in trotting; swimming in circles instead of straight), or to use its healthy legs for unusual tricks, or finally to return limbs with arbitrarily disarranged muscles to their usefulness as instruments

of locomotion. There would seem to be no reason why an animal which can change the rhythm of its several limbs in the acts of locomotion should not be equally adept at changing the rhythm of the several muscles within the limb, if in last analysis it all comes down simply to rearranging neurone linkages.

However, this view is strictly contradicted by the facts. We have already quoted evidence to show that in amphibians, and even in the rat, the time order according to which muscles execute a limb movement is unalterably fixed, while at the same time the total behavior of these animals is amenable to reconditioning by training and other regulatory adjustments. In the light of these facts, the distinction between rigid and plastic functional levels assumes great significance, as indeed the neglect of the hierarchical principle would lead, and has led, to serious confusion. In brief, the fact that an animal can learn (on levels S and W) to use its limbs differently in moving the body, does not necessarily imply that it can likewise learn (on levels O and G) to use its muscles differently in moving a limb. Adaptive functional reorganization is a prerogative of certain functional levels only. Therefore, in raising the question of learning separately for each level, we merely give expression to realities.

The preceding pages may suffice to bear out our contention, that progress in the study of coordination has been held up both by lack of restraint in extrapolating from higher mammals to animals in general, and by lack of precision in the application of the principle of learning. In this state of affairs it is not surprising to find the changes observed after tendon transposition, muscle transplantation, nerve crossing, sectioning of central tracts, destruction of brain portions, and similar interferences, lumped together indiscriminately under the common headings of re-education, regulation, functional restoration, reconstitution, reparation, re-organization, re-adjustment, and the like. If in the future more discretion will be exercised in the use of these terms and if the mere statement, that a behavioral change has occurred, will be amplified by precise information as to what this change has consisted of and where it has taken place, the gain for our understanding of nervous function will be enormous. Then,

all the mentioned interferences, instead of merely serving to tell whether or not "functional recovery" can occur, become discriminative assay methods, revealing the degree in which the various functional levels participate in the noted "adjustment." It is in this assaying capacity that the transplantation experiments to be discussed below have been used, and since this method invites much wider application, a few comments on general methodology seem appropriate.

Experimental methodology

As all biological experiments, those dealing with the nervous system fall essentially into three classes: *defect* experiments, *isolation* experiments, and *recombination* experiments (compare *P. Weiss*, 1939, p. 147 f.). Given a system Y, consisting of parts A, B, C, D, and so forth, the experiment aims at establishing the relations between the system and its parts, as well as among the constituent parts, by severing the existing relations. Singling out, for instance, part A, the *isolation* experiment determines the properties and capacities of A, when completely released from the rest of the system, while the *defect* experiment, complementary to the former, ascertains the properties and capacities of the remainder of the system (Y minus A). In both cases the relations between the system and the part are permanently interrupted. It is left to the *recombination* experiment to supply the positive part of the story by restoring connection and relations between the severed components, however, with such added variations from the original condition that it will be possible to discern whether and in what respect the relation between the system and part A differs from its relation to parts B, C, etc. Part B is supplanted for part A, and vice versa, and the subsequent conduct of the altered system is studied. If the system behaves as before, we conclude that A and B are equivalent; if it behaves differently, the change is ascribed to the differential between A and B.

In the past the defect experiment has been by far the predominant method in the study of nervous function. The value of the isolation experiment is increasingly appreciated; witness the work on isolated nerve fibers and isolated brain parts. The

recombination experiment, however, has been largely neglected. Yet, its instructiveness greatly exceeds that of the defect experiment. In what respect, can be easily shown.

Let us quote an example. We cut a tendon and note subsequently that limb movements are changed. Now, the operation has altered a number of conditions in one stroke: It has caused trauma, produced a gap in the elastic continuity of the tissues, interrupted the transmission of pull from muscle to skeleton, abolished stretch reflexes from the affected muscle, and, as a result, changed the mechanical and innervatory balance of other muscles. What each one of these factors contributes to the common defect, cannot be immediately discerned. Their effects can be separated, however, by resuturing the tendon stumps in various modifications—under slack or shortening; to the old muscle or to an antagonist; with or without concomitant denervation of the muscle;—in other words, by restoring certain, but not all, of the severed relations.

Similarly, peripheral nerve section leads to a complex functional disturbance the net effect of which—impairment of motility—does not reveal its composite nature. The locomotor apparatus suffers changes which are partly due to the trophic and mechanical by-products of muscle denervation in general, partly to the fact that a particular nerve (not just innervation in general) has been lost, partly to compensatory reactions of other muscle centers not directly affected by the operation, with every one of these effects telling on all levels, from the simple movement of a joint up to the aimed behavior of the animal as a whole. We know that all these factors enter into the result, but only systematic recombination experiments, consisting in this case of the replacement of old peripheral nerve connections by new ones, with or without rearrangement of tendons, can help us to disentangle them. Only then can we learn what difference it makes whether a muscle is merely supplied with nerves or actually receives impulses; whether it is just innervated or innervated by one particular nerve rather than another; whether it merely contracts or actually moves the skeleton; whether it operates the skeleton to good use or in a mechanically inefficient

manner; whether it contributes to an act of biological significance or one that runs counter to the interests of the organism as a whole.⁵

The rest of this article is essentially a detailed account of how problems of this kind, refractory to other methods, could be solved by recombination experiments. It is hoped that the advantages of the method will become sufficiently evident to encourage its extension to problems not yet hitherto tackled, but entirely within its reach. In view of the fact that endocrinology owes much of its rapid progress to the introduction of routine transplantation methods, it is surprising that neurology has not yet adopted analogous methods to any appreciable extent.

AN ANALYSIS OF COORDINATION IN AMPHIBIA

The principle of myotypic response ("resonance")

Transplantation experiments carried out after the scheme just outlined have led to an astonishing discovery concerning the manner in which the central nervous system controls the musculature. They have revealed a series of phenomena, commonly referred to, somewhat vaguely, as *resonance* phenomena, which have provided us with an assay method of nervous function of much greater discriminatory power than any other method available. Since the results obtained with this method have been amply reported and reviewed on previous occasions, only those points will be recapitulated here which have an immediate bearing on the problem of this paper. For the latest comprehensive review, see *P. Weiss*, 1936b.

Reduced to the simplest formula, the results have shown that muscular control is based on a principle of selective correspond-

⁵ The lack of distinction in these matters is clearly reflected in the anatomical nomenclature of muscles. Some muscles received their names from some constitutional characteristics, such as shape, (e.g., m. trapezius; m. piriformis), some from their topographic relations (e.g., m. intercostalis; m. subscapularis), some from their skeletal connections (e.g., m. ileo-fibularis; m. coraco-brachialis), some from their kinetic effects (e.g., m. levator scapulae; m. extensor carpi; m. corrugator supercilii), some from their biological function (e.g., m. masseter [from *μασσαι εσθαι*, to chew]; m. risorius, the "laughing" muscle; m. vocalis, the "vocalizing" muscle).

ence between nerve centers and individual muscles, which enables the centers to identify and engage any given muscle by its *name*, irrespective of its mechanical effects, or the biological effects which the latter have for the body as a whole. Each individual muscle owns some distinctive constitutional characteristic through which it is differentially distinguished from all other normal muscles. By virtue of this distinctiveness, in a manner not directly discernible from the experiment, the central nervous system can discriminate muscle from muscle, regardless of where they are attached and how they act. Each muscle is centrally represented by units of corresponding specificity, and these, in turn, are activated by the centers selectively. The basic elements of motor function are specific central "calls," one for each muscle, and each so organized that it will affect just those motor units belonging to the appropriate muscle, and no others. The totality of "calls" at the disposal of the centers represent the code or vocabulary, as it were, of which all central messages are necessarily composed.

All of these statements contain no hypothetical implications, but represent merely the net result of a large series of experimentally established facts. Presented in a nut shell, the underlying facts are the following.

In amphibians⁶ it is possible to graft an extra muscle, or group of muscles, or even a whole limb, to the body wall and provide motor innervation for these transplants by diverting to them some motor nerve branch from one of the normal host limbs. The amount of deviated nerve fibers can be held to such insignificant proportions that no perceptible change in the function of the host limb results. This small nerve source is fully adequate to assure complete re-innervation of the transplant, inasmuch as nerve fibers in the course of regeneration undergo profuse branching. The experiments were so devised as to insure that the muscles of the transplants would be re-innervated for the most part or wholly by nerves with which they formerly had no relations.

⁶ All statements in this chapter relate only to young amphibians, for which direct experimental proof is at hand, while the possibility of extending the results to other groups will be discussed later.

After transmissive connections between the regenerated nerve fibers and the grafted muscles had been restored, the supernumerary muscles began to exhibit regular and strong contractions whenever the host limb, from which their nerve supply was derived, moved.

The stress, however, lies not so much on the fact that the transplanted muscles had become re-engaged in functional activity, but on the peculiar time order in which they were found to operate. Extensive studies, under a great variety of conditions, of the precise times when a transplanted muscle starts to contract and ceases to contract, as well as of the degree of its contractions during that active period, has revealed a principle of such definiteness and constancy that it amounts to a law. *The phases of activity of an extra muscle correspond precisely to the phases in which the muscle of the same name, or synonymous muscle, is found to be active in the host limb innervated from the same plexus of the spinal cord.* Whether the transplant consists of a single muscle, or a group of muscles, or a whole limb, each individual muscle as such duplicates the action of the synonymous muscle in the normal limb nearby. This phenomenon has been described, and ever since been referred to, as "*homologous response*" of *synonymous muscles*. The term signifies that if a body district is provided, instead of with a single muscle of a given kind, with two, three, or even four homologous muscles of the same kind and name, all of them will act in unison, the contractions beginning at the same moment, developing the same proportional tension and subsiding at the same time; the only prerequisites being, that all of them receive their nerve supply from the same side and the same general level of the spinal cord (e.g., limb level in the case of limb muscles), and that the transplantation be done in young, preferably premetamorphic, animals.

Now, what does this phenomenon of "*homologous response*" actually mean? In spite of the ample attention given to it in the past, it does not seem that the majority of authors have succeeded in seeing it in the correct light. Many authors, while reporting the phenomenon correctly, yet have missed the point which it so clearly proves, namely, the existence of correspond-

ences of a specific kind between nerve centers and individual muscles. Thus, the phenomenon was variously described as demonstrating almost unlimited "plasticity of coordination," "learning capacity" even at the lowest level, "adjustments" of the nervous system to the introduction of a new organ, integrative action of the spinal cord, and so forth. Some of these interpretations are strictly incorrect, others merely besides the point. It would be idle to try to fix the blame for these misinterpretations. Part of it can probably be ascribed to lack of clarity in the earlier descriptions of the phenomenon, as well as to misleading terminology, part to the fact that the problem of coordination, to which the phenomenon offered a clue, was not usually presented in the correct light. It thus becomes necessary once more to explain the intrinsic meaning of this phenomenon of "homologous response" of synonymous muscles.

To begin with, to emphasize the fact that the transplanted muscle and the synonymous muscle act in unison, is already putting the wrong slant on the phenomenon, because in stressing the association of the two peripheral parts, we give prominence to a rather irrelevant aspect. We make it appear as if the transplanted muscles, or rather their centers, had in some way learned to imitate the synonymous normal muscles, even though it would be difficult to find any plausible reasons why they should have done so. To avoid this misconception, it must be stressed that in all these experiments the normal muscles simply serve as *indicators* of the hidden activities of the central nervous system, and that their actual presence is in no way required for the appearance of the phenomenon. Even if all muscles of the normal limb are removed, the transplanted muscle keeps on functioning at precisely the phases when the removed synonymous muscle would have functioned if it were still present. But so long as a normal limb is available, we use it as a detector to tell us which combination of muscles the central nervous system tends to activate in any given phase of a locomotor act.

Let us now forget for a while that effective limb movements are of service to the animal, and let us consider them simply in their service to the observer as convenient instruments for the

visible registration of the content of the varying central commands. To be used for this purpose, movements must be resolved into muscle actions. This can be done directly or indirectly—directly, by connecting the muscles with mechanical or electrical recording devices, as is commonly done in the study of simple reflexes with fairly constant stimulus-response relations; indirectly, by taking cinematographic records of the movements and reconstructing the muscular activity from the measurable changes in the angles of the various joints. The former method, besides being limited to animals above a certain size, has the disadvantage of interfering with the free execution of the movements, while the latter falls short in two respects: firstly, it fails to register isometric contractions, which produce tensions to overcome resistance without producing excursion of the joints, and secondly, the individual muscular contributions to the movement, instead of self-registering, are only indirectly recoverable from the record. In the work on homologous response both kymographic registration and slow-motion picture analysis have been employed. However, experience has shown that the former method is by far less suitable in the study of coordination, because the technical measures necessary for direct muscle registration (anaesthesia, strapping, fixing of joints, tendon dissection, etc.) interfere with the execution of most of the regular locomotor repertoire of the animal to such a degree that little insight into the normal, unrestrained, performance can be gained.

By slow-motion picture analysis, a complex movement can be resolved into the constituent muscular actions: the sequence in which different muscles become dominant and the duration of their phases of activity can be determined. We thereby obtain a time record—a chronological “*score*,” as it were,—of the central activities through which the various muscles are engaged. Thus, when the forearm is bent against the upper arm, we take this to signify that the central nervous system has set into action chiefly that group of neurones which innervates the biceps muscle. As the flexion slows down and finally reverts into extension, we interpret this as central activation of the triceps neurones. Doing this for all muscles involved in a given movement, we compose

a master chart of the central timing mechanism effecting the movement. Such a master record of coordination may be compared to the score of a piece of orchestral music in which onset, intensity, and duration of each instrumental part are recorded separately, except that in the case of our muscular orchestra no instrument can produce more than a single tone: contraction of the particular muscle. It is herewith proposed to call this master time record of all muscles participating in a given movement, the *myochronogram*.⁷

As a concrete example on which to carry on our further discussion, we reproduce here the myochronograms of the fore limbs of a salamander in the act of walking on solid ground (fig. 4). For the sake of simplicity, only the shoulder and elbow joints are included, while the wrist has been ignored. Furthermore, only six major muscles have been selected as representatives. These muscles are shown in a dorsal view of the left fore limb in the inset (upper left of the figure). The shoulder muscles chosen are the same as those of figure 2, namely, an elevator (Δ), an abductor (\circ), a depressor (\times), and an adductor (\bullet). The upper arm muscles are represented by a flexor (\square) and an extensor (\blacksquare) of the elbow. The two central strips of the picture reproduce in diagrammatic outline six phases of one complete walking cycle, both in dorsal and rear views. Double rings indicate the fixed points on the ground in which the animal sets down its wrist and around which as pivotal points it swings the body forward. The muscles in action are marked by their respective symbols.

In phase 1, the right hand takes hold on the ground (\odot), and presently (phases 2 and 3) depression (\times), adduction (\bullet), and extension (\blacksquare) of the right arm swing the animal forward, while

⁷ Although we have used myochronograms as the simplest method to represent movements for years, none were published until 1937 (*P. Weiss*, 1937d). Coincidentally, it was then found that a Swiss surgeon, *Scherb* (1938), had been using the same method to symbolize movements under the name of "myokinesiogram." If we give preference to the term "myochronogram," it is only because "*kinesiogram*" means record of motion, whereas "*chronogram*" simply means "time record," thus providing for the inclusion of non-motile, isometric, muscle contractions.



FIG. 4. MYOCHRONOGRAM OF THE FORE LIMB ACTION OF A SALAMANDER DURING AMBULATORY PROGRESSION OVER SOLID GROUND
Detailed explanation in text

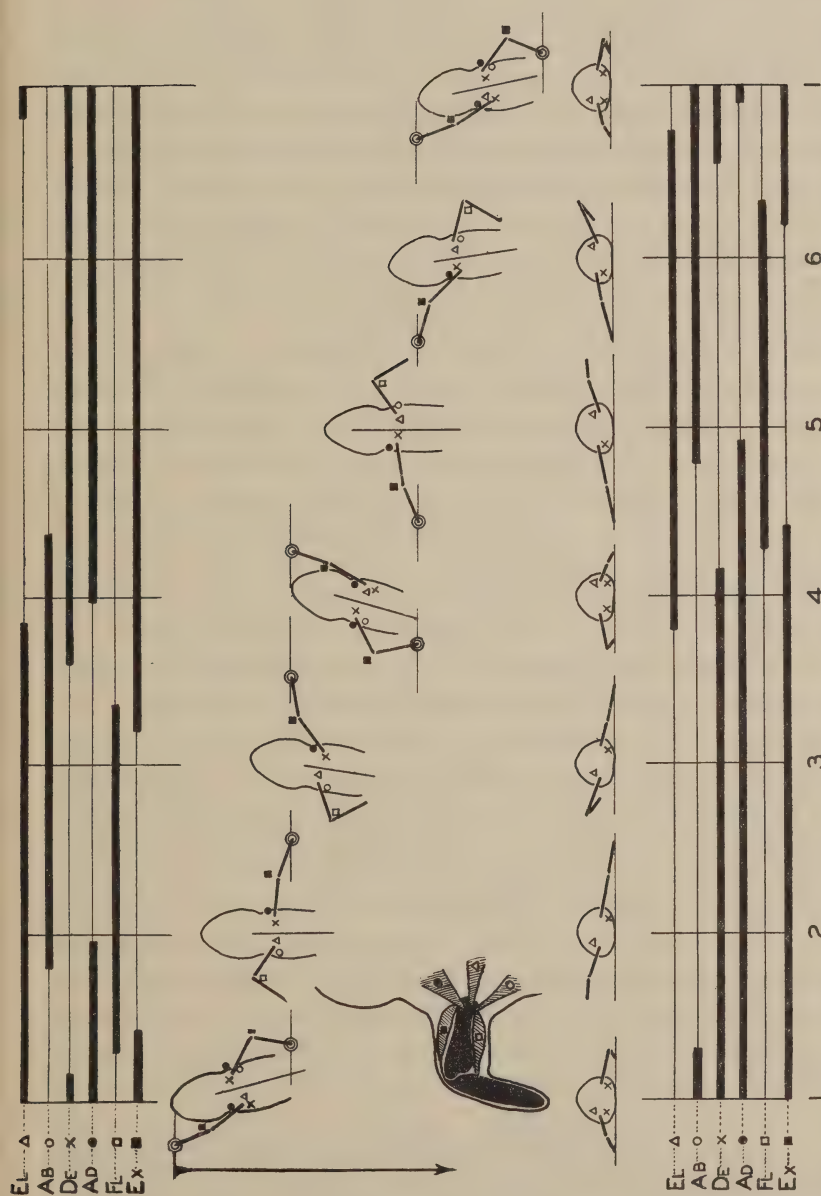


FIG. 5. MYOCHRONOGRAM OF THE FORE LIMB ACTION OF A SALAMANDER WITH INTERCHANGED, I.E., REVERSED, FORE LIMBS DURING AMBULATION
Detailed explanation in text

at the same time the left arm is lifted (Δ) and brought forward (\bigcirc , \square). At point 4, the left hand is then set down and serves as pivot around which the body is swung forward through phases 5 and 6, while the right arm in turn is lifted from the ground and brought forward. The top and bottom records represent the myochronograms of the left and right fore limbs, respectively, as each goes through the illustrated phases. In order to be truly representative of the resulting movements, these records should include an account of the varying intensities of the muscular contractions. Since these cannot be directly observed, we confine ourselves to the score of time relations.⁸

With sufficient practice it might be possible in reading a myochronogram to visualize the resulting movement just as an orchestra conductor reads a musical score. To determine how much variation in detail there is in the play of muscles in moving a limb, would require much more intimate studies. However, all observations thus far concur in demonstrating that the essential chronology expressed in the above myochronograms is typical. Walking is always effected by the same general sequence of muscular activities in all animals of the same species. In other words, the muscular integration on the level O of our hierarchical scale (p. 24) follows a stable chronological pattern.

Compared with this firmly set pattern for a given limb, the association between left and right fore limb, or that between fore limbs and hind limbs is much looser. That is to say, while usually in undisturbed and vigorous walking the alternation between left and right limb is as strict as is indicated in the diagram, amounting to a shift of the lower myochronogram against the upper one of just one half phase of the whole cycle, there is also frequently independent action of the two limbs, either one moving without the other, or both moving in phase, or even both entirely out of turn. Similarly, in vigorous walking there is usually an intimate correlation between the movements of hind limbs and

⁸ The overlap between antagonistic muscles in these diagrams has not been directly observed, but put into the records in accordance with the work of *Wachholder* (1923), showing that the contraction of antagonistic muscles actually sets in some time before the reversal of the movement becomes visible.

fore limbs in that the adduction phases, and likewise the abduction phases, of diagonal limbs coincide. However, at other times, the hind limbs operate independently of the fore limbs, and dissociation between the two pairs of limbs occurs even more commonly than between the two partners of the same pair. Simple observation thus demonstrates that integration on the level S of the hierarchical scale is subject to much greater variation than that found on the lower level O.

Now, let us return to the animals with supernumerary limbs. The movements of their normal limbs serve us to construct the central myochronograms, that is, to reveal what muscles are centrally being called up for action at any given moment. Suppose we examine an animal with an extra limb attached to the left fore limb plexus. The myochronogram informs us that in phase 3 the left fore limb centers discharge impulses destined to engage the elevator, the abductor, and the flexor of the elbow. Watching the transplanted limb, we note that in it, too, out of the whole extra set of muscles, just the elevator, the abductor, and the flexor respond, i.e., precisely those muscles provided for in the central score. Since this holds for all muscles and at all times, we must conclude that the centers, in a sense, "call up" the individual muscles by their names; further, that when the name of one muscle is called, all muscles of that name react, which implies that each muscle is endowed with some peculiar property enabling it to respond to the calls of its own kind selectively. Thus, the "homologous" response of supernumerary muscles signifies a selective correspondence between central impulses and peripheral effectors rather than a tie-up between synonymous muscles as such.

Obviously, the designation of the phenomenon as "homologous response" has been misleading in that it places the emphasis on a technical rather than on an essential feature. For this reason, it would seem more to the point to speak of a principle of "*myotypic response*," which means "muscle-specific response." This change of terminology is herewith proposed.

The factual content of the phenomenon of myotypic response can be reduced in essence to two points: (1) The protoplasm

of each individual muscle has a specific and distinctive constitution, distinguishing it from all other muscles. (2) This constitutional specificity is instrumental in establishing a selective relation between the centers and any muscle of that particular kind of specificity.

This formulation merely expresses logical conclusions to be drawn from the observed facts. It contains no reference whatever as to just how those specific relations between muscles and centers are effected. If we are to consider this latter question, we find ourselves no longer on the same solid ground as before. Here the experiments fail us. While they have set up a definite frame within which any explanation of central-peripheral correspondence must hold itself, they carry no further positive suggestions. The tentative explanation currently favored and presented in the following pages should, therefore, be considered as entirely hypothetical. Whether or not it will ultimately prove to be correct, does not affect the validity of the principle itself. The reality of the myotypic principle remains a fact, its mechanism a matter of further research.

Three possible mechanisms suggest themselves.

(1) One might assume that there are as many specifically different types of motor cells in the spinal cord as the corresponding peripheral district contains specific individualized muscles, and that in development, as well as in regeneration, each muscle receives exclusive and selective innervation by fibers of the corresponding type. This would presuppose either selective attraction by the muscle of its appropriate kind of nerve fibers from a distance, or some scheme by which nerve fibers, though growing out indiscriminately, would be admitted for functional connection only into those muscles which are of the corresponding type. As one can readily see, this assumption would match the qualitative differential among muscles by a corresponding inherited qualitative differential among the central cells. It would further postulate an unailing capacity of the two predestined partners of each nerve-muscle pair to find each other. The possibility that such strict selectivity might prevail in the innervation and re-innervation of muscles has been definitely ruled out by ex-

tensive studies on nerve regeneration in general, and specifically in reference to the experiments on supernumerary muscles and limbs (*P. Weiss*, 1937b). We know for sure that regenerating nerve fibers coming from whatever source can form functional connections with whatever kind of muscle. Thus, any explanation of myotypic response on the basis of selective fiber regeneration can be definitely excluded. We, therefore, pass on to a second possibility.

(2) The excitation produced in the centers might be of composite character, that is, have a different pattern depending on which muscles it is to set into action. The resultant excitation would be dispatched over all motor fibers of the district, delivered to all the muscles and there be analyzed by the end organs, each one picking out its proper component. This interpretation of myotypic response, tentatively introduced in the early reports on the phenomenon (*P. Weiss*, 1924) had to be later abandoned as not consonant with the subsequent development of nerve physiology, in that it has since been fairly securely established that no excitations are electrically demonstrable in motoneurons unless the muscle, too, is active (see *Wiersma*, 1931). In other words, the motor impulses are already assorted before they leave the centers, and the idea of an analysis of the impulse pattern by the peripheral end-organs can no longer be entertained. It is somewhat disconcerting to see that some authors, ignoring the explicit renunciation of this idea (*P. Weiss*, 1934, 1935a, 1936b), still insist on presenting it as the official version, as it were, of the resonance principle. At present, the view best in agreement with the facts, although not entirely free from difficulties, is the following.

(3) Each muscle exerts, by virtue of its individual protoplasmic specificity, a correspondingly specific influence upon its motor nerve fibers. As a result, the nerves acquire specific differentials which match and centrally represent the variety of muscles. The biceps muscle, for instance, would gradually transform any nerve fibers connecting with it into strictly biceps-specific fibers; that is, it would impress upon them some biochemical tag through which they become centrally recognizable as belonging to the

biceps muscle. This hypothetical process of appropriation of the motoneurons by their muscles has been called "*modulation*" (P. Weiss, 1936b).

According to this view, a nerve fiber which has been severed from its erstwhile connections and switched to another muscle, would lose its former specificity and acquire the new one. However, it seems that nerve fibers, as they grow older, lose their plasticity and become irreversibly ingrained with the specificity of the muscle with which they had been connected up to then. This is the reason why myotypic response of transplanted muscles is not observed unless the transplantation is performed below a certain critical age of the animal. If a nerve is switched to a new muscle after that critical period has passed, the muscle will always contract in the phases in which the old muscle, which used to be at the end of this nerve, would have contracted. In other words, the response has become *neurotypic* (nerve-specific), the nerve specificity being the residue of muscle-specific effects received during an earlier phase of life. A *young* nerve, which after connecting with a biceps muscle had become biceps-specific, can still be transposed to a triceps muscle and assume triceps-specificity, thus changing its central tune. However, after having been under prolonged influence of its original biceps contact, transfer to the triceps has no longer any retuning effect, and the nerve responds centrally as if the muscle at the end were still a biceps.

That the muscular specificity is conveyed to the spinal centers through "*modulation*," i.e., an ascending process extending over the motor neurones directly, rather than through the mediation of sensory fibers, is proved, apart from other evidence, by the fact that it occurs just the same after the sensory innervation has been eradicated (P. Weiss, 1937c). It seems reasonable to assume that modulation embraces the whole motor neurone, including the ganglion cell. But whether it extends farther centrally, and how far, remains to be determined.

Modulation itself being largely hypothetical, it would be idle to indulge in speculations about its nature. Merely as a matter of personal preference, I like to view it as belonging to the class

of specific molecular adaptations of which the immunological reactions are the most familiar example. Just as an antigen calls forth in the cell the production of a correspondingly shaped antibody, so the specific biochemical factor of each muscle may provoke in the nerve protoplasm complementary molecular configurations, beginning at the myo-neural junction and from there spreading through the neurone much in the fashion of virus reproduction. The molecules thus molded may be essential links in the chemism of the "propagated disturbance," or they may be merely indirectly related to it. In either case, their biophysical and biochemical parameters would determine the selectivity of the response of the whole unit to the biophysical and biochemical activities of its central environment. Evidently, this is one way to explain the fact that the central response of each motoneurone is selective in accordance with the identification mark of its muscle. However, there is no evidence that this hypothesis comes anywhere near the truth, and we mention it only to prove that a rational explanation of modulation is not beyond reach.

The process of modulation sheds no immediate light on the central mechanism of coordination. Modulation merely furnishes the centers with necessary clues without which central coordination could not become peripherally effective. For if central coordination, as we now realize, operates in terms of individual muscular specificities, the centers need means to identify the various muscles. Modulation provides them with such means of recognition. However, the fact that the motor ganglion cells, after undergoing modulation, represent an assortment of elements with distinctive characteristics, in no way explains the central mechanism through which these elements are being activated in the varying combinations provided for in the central "scores" of coordination. Is it that dendritic links develop among ganglion cells of the same "tune" through which all cells belonging to the same muscle would become connected into a unitary system responding to impulse patterns from higher stations as an entity; or is the association based on purely physiological affinities of a sort that would force all those elements which have the same selective specificity to respond in unison to a common activating

agent of corresponding specificity? This is a problem which furnishes much food for speculation but little hope of solution in our present state of knowledge.

It seems preferable, therefore, to refrain in this place from a further consideration of these hypothetical matters and to concentrate on the consolidated factual content of the principle of myotypic response, which is that *the central nervous system, in dealing with the musculature, utilizes specific means through which each muscle can be called into action independently, in accordance with its individual constitution.*

Myotypic response as assay method

Having established the general validity within known limits of the principle of myotypic response, we may pass on and use it to assay central coordination. How, will become clear from the following example.

Let us consider what is conventionally described as a *flexion reflex*. A stimulus applied to the toes results in the withdrawal of the foot and leg. This reaction occurs essentially on the level O of the hierarchical scale, implying operations on the subordinate levels G, M, and N. In terms of the top level (W) it can be rated as part of an escape reaction from a harmful stimulus. In terms of the organ level (O) it means approximation between base and tip of the limb. In terms of the muscle group level (G) it amounts to reducing the angles of the ankle and knee joints, commonly designated as "flexion"; and on the level of the individual muscle (M), it simply means contraction of those muscles which happen to be inserted on the flexor sides of the joints (e.g., hamstring at the knee; tibialis at the ankle). Our problem is to decide in terms of which of these levels the coordination of the withdrawal reflex is laid down.

It is here that the assay function of a transplanted muscle can prove its value. For we can transplant a flexor muscle in such a fashion that it will have the mechanical effect of extending the joint instead of flexing it, as it did before. Thereby we alter the relation between the M level and all higher levels. When a "flexion" reflex is now elicited, will the response still be "flexion,"

or will it be a contraction of what used to be the "flexor" muscle now producing extension? Or we can change the insertion and orientation of a whole limb with regard to the body in such a manner that, while the muscles will continue to produce the normal kinetic effects within the limb, the net result of the limb action for the body as a whole will become quite different from what it was before. Thereby we upset the relation between level O and the higher levels S and W. Will coordination patterns within the limb thereafter remain as they were before, perpetually in discord with the needs of the body, or will they be remodeled and re-integrated with the levels S and W so as to restore harmonious operation of the whole?

Applying these experimental tests, it was found that a stimulus which normally yields a "flexion" reflex will invariably lead to a contraction of the hamstring muscles and the tibialis group, no matter whether the resulting movement comes actually out as flexion, or, owing to transposition of the muscles, as extension, rotation, or any other joint excursion, irrespective also of whether or not the resulting flexion or extension, as the case may be, leads to an effect which can be considered adequate from the standpoint of the body.

Now, let us go one step farther. Let us change the nerve supply of a "flexor" muscle, either one that still flexes, or one that has been transposed to the extensor side, by substituting an "extensor" nerve for the original "flexor" nerve. As we have outlined before, the result will vary with the age of the animal at the time of operation. Late operations will lead to neurotypic response; that is, the muscle will contract during "extensor" phases only and, hence, not take part in the "flexion" reflex (cf. *Sperry*, 1941). Early operations, however, will result in continued myotypic response, that is, a "flexion" reflex will bring in the "flexor" muscle even though it is now innervated from an "extensor" nerve and may have been switched over to the extensor side so as actually to produce extension. Whatever we do to it, the muscle with the "flexor" constitution will be the one to respond in the so-called "flexion" reflex (see table 2).

Such being the situation, it would seem much more to the point

to speak of a "tibialis-semitendinosus-semimembranosus" reflex, rather than of a "flexion" reflex, and to describe the "flexion" reflex about as follows: A stimulation of sensory fibers from the skin of a toe sets off a central discharge pattern, which selectively engages all motor neurones, however much scattered over the central district, which bear the specific "tibialis," "semitendinosus," and "semimembranosus" tags previously acquired from

TABLE 2
Effect of a "flexion" reflex on a "flexor" and "extensor" muscle before and after tendon crossing or nerve crossing or both

OPERATION	OPER- ATED MUSCLE	INSERTION		INNERVATION		REFLEX EFFECT	
		Left on	Transferred to	Original	Transposed	Contracting muscle	Kinetic effect
Control	Flexor Extensor	Flexor side Extensor side		Flexor nerve Extensor nerve		<i>Flexor</i>	Flexion
Tendon crossing	Flexor Extensor		Extensor side Flexor side	Flexor nerve Extensor nerve	*	<i>Flexor</i>	Extension
Nerve crossing*	Flexor Extensor	Flexor side Extensor side			Extensor nerve Flexor nerve	<i>Flexor</i>	Flexion
Tendon and nerve crossing*	Flexor Extensor		Extensor side Flexor side		Extensor nerve Flexor nerve	<i>Flexor</i>	Extension

* Nerve crossing prior to loss of ability of re-modulation.

their respective muscles, with no regard to the actual kinetic and biological effects of the resulting contractions. The fact that the contraction of the hamstrings produces flexion, which has given the reflex its name, is, physiologically speaking, pure coincidence; fortunate from the standpoint of the animal and, of course, fixed by virtue of that very fact during the phylogenetic evolution of the species, but entirely dependent on the skeletal attachments of the muscles being and remaining what they are. If we disrupt this anatomical wisdom, we note no tendency of the centers to maintain the integrity of the response in terms of its effect ("flex-

ion," "withdrawal"), but a blind continuance of the inherited central impulse scheme, delivered in terms of muscle-specific calls, in spite of the adversity or, at best, indifference to the individual of the resulting effects.

We have chosen a reflex as our first example because reflexes are usually conceded to be sufficiently rigid to fit into this picture. Therefore, the statements of these last pages do not exact much revision of current thinking, except in so far as they show that the response called for in a given reflex is not due to firmly set central connections, but that the nerves are conditioned for their response by their muscles. All the other conclusions could have been reached without knowing about the myotypic principle. It is only on the level of the more complex motor activities that uncertainties arise which it might not have been possible to clear up without the aid of the myotypic test.

A transplanted supernumerary limb can be of no use to the body unless possibly in the very special case where it has been inserted exactly in the same orientation as the near-by normal limb so that the pair can execute parallel action. In all other cases the actions of the transplant are sheer waste from the standpoint of the body. Conditions can even be created in which the action of the transplant is distinctly harmful in that it counteracts the normal limb (*P. Weiss, 1937a*). No adjustment or elimination of the wasteful action has ever been observed. It was suggested by *Bethe and Fischer* (1931, p. 1119) that the disturbance caused to the animal by the extra limb might not have been sufficiently vital for the centers to do something about it. It was argued that so long as the host limbs could continue in their normal function, the incentive to change the functional pattern might not have been strong enough. This criticism, however, has been invalidated by later experiments in which the nuisance value of the transplant was so aggravated that it created a serious predicament for the animal. Since these experiments illuminate the problem of coordination most clearly, we shall recount them here briefly, adding a number of comments that were not contained in the original publication (*P. Weiss, 1937d*).

Unmodifiability of locomotor scores

In larval salamanders possessing developed and functional limbs, the two fore limbs were mutually exchanged under preservation of their original dorsoventral orientation. Since the two limbs are mirror images of each other, this operation amounts to replacing one limb by another limb which has the same assortment of muscles but in exactly the reverse arrangement. A comparison between the insets of figures 4 and 5 explains the situation. Of the six muscles which represent the limb in our myochronograms only the elevator (\triangle) and the depressor (\times) have retained their normal positions relative to the body, while the adductor and abductor of the shoulder, and likewise the extensor and flexor of the elbow, have traded places. Adductor (\bullet) and extensor (\blacksquare) now lie at the anterior instead of the posterior border of the limb, and abductor (\circ) and flexor (\square) lie on the posterior instead of the anterior side.

After being re-innervated by regenerating nerves, these limbs resume function. The characteristics of this function are outlined in figure 5, in which six phases of a full walking cycle have been reproduced diagrammatically. Strips of the moving pictures from which these diagrams were reconstructed have been reproduced previously, and the reader may be referred to the earlier publication (*P. Weiss*, 1937d) for further details. The functional effect of the anteroposterior reversal of the whole muscle apparatus was so obvious that it seems hardly necessary to add much to the story, as it unfolds itself in a comparison between figures 4 and 5 (pp. 34, 35).

All movements of the trunk, hind legs, and other parts which have been left untouched by the operation, are identical with those of a normal animal in the act of progression. Hence, we can use these normal parts to identify the successive phases of locomotion and to line them up with the corresponding phases of the normal animal in the diagram. This being done, we realize immediately that the positions of the transposed fore limb (fig. 5) and those of the normal fore limb (fig. 4) are precise mirror images for each corresponding phase of the body movement. If we re-

solve the movement again into its component muscular contractions, we note that at any one moment the combination of muscles active in the reversed limb is identically the same as the one that would be active at that particular moment if the limb were a normal unreversed one. Using the myochronogram as index of the central impulse pattern, we thus learn that the centers have continued to call up the individual muscles in the same rhythm, sequence and intensity as they had done when they were still operating normal legs with unreversed musculature. In doing this, however, they lead to peripheral effects which are exactly the opposite from what would serve the organism: instead of progression, they produce regression. This is explained in the diagram (fig. 5).

In phase 1, the reversed limb on the right side has taken hold on the ground. During phases 2 and 3 the extensor (■) and adductor (●) muscles contract—the same muscles which are active during phases 2 and 3 in the normal animal (fig. 4). This swings the body backwards (see arrow), while the arm on the left side reaches backward owing to the contraction of its elevator (△), flexor (□) and abductor (○). In phase 4, this free arm, in turn, takes hold on the ground, and the following contractions of its adductor (●) and extensor (■) bring the body still further backward through phases 4, 5, and 6. Thus, the muscles of the reversed limbs, while going through precisely the same cycle of innervation which their synonymous muscles would go through in the normal limbs, move the body backward instead of forward.

Actual regression occurs only if other means of progression, such as the tail and hind limbs, have been removed or paralyzed. If the hind limbs are present, however, the resultant effect is a constant struggle between the hind limbs and the fore limbs, the former striving to advance the body and the latter cancelling the effect by moving the body backwards by the same amount. The net result is that the animal swings back and forth without ever moving from the spot. It is almost pathetic to see how helpless the animals are about their predicament, and although some of them have been kept for more than a year, long beyond metamorphosis, their behavior has never changed.

From these results it must be concluded that the centers operate in terms of individual muscle calls which are combined into definite groupings so pre-arranged as to yield suitable effects in an animal with normal distribution and normal attachments of its muscles, and that the centers continue to operate according to the old scheme even when the peripheral anatomy is no longer normal and the central design no longer yields the desired peripheral effect. Paraphrasing the situation, one might say that the centers continue to act under the illusion that they are still operating a normal limb with consequential results.

In these experiments, the objection certainly no longer holds that the disturbance of behavior was not sufficiently crucial for the animal to attempt an adaptive change. If they could not improve under these conditions, then they surely cannot under any circumstances. There is one possible objection, however, that deserves some consideration. It might be submitted that the anatomical conditions in the reversed legs might have been such as to preclude, for purely mechanical reasons, an effective participation of these limbs in forward locomotion, in which case even the highest power of central re-education would have been able to do no good. To this one could simply reply that in that case the animal might, at least, have learned to suppress the activities of the useless fore limbs altogether, which undoubtedly would have been of some help. However, there is more pertinent evidence on hand to invalidate the mechanical argument. It lies in the observation that animals with reversed fore legs can actually exert forward traction through these limbs. This happens whenever the animal as a whole tends to retreat.

Figure 6 explains the case. The upper half of the picture shows the fore limb coordination of a normal animal which tries to recede from a repulsive stimulus; for instance, strong ammonia vapor, or a moving object of threateningly large dimensions. The essential mechanism consists of adducting (●) and flexing (□) the elevated (△) arm, then setting it down on the ground (×), and finally extending (■) and adducting (○) it, with the result that the body is thrust backward. Usually the right and the left arm alternate, as is shown in the myochronograms at the top and

bottom of the figure. A comparison of these myochronograms with the records of forward locomotion in figure 4 reveals that the essential difference between the two types of movement is the change in the phases in which the abductor and adductor muscles

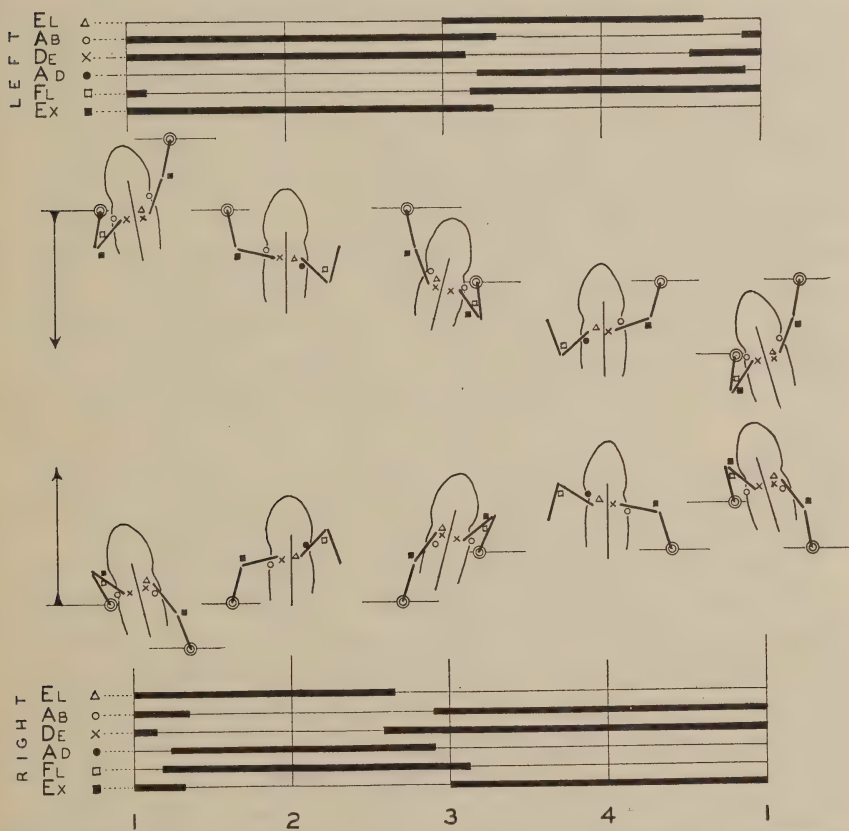


FIG. 6. MYOCHRONOGRAM OF FORE LIMB ACTION DURING "RETREAT," EXECUTED BY NORMAL SALAMANDER (UPPER CENTER) AND SALAMANDER WITH REVERSED FORE LIMBS (LOWER CENTER)

For details, see text

come in. In both movements the elevator and flexor muscle, and likewise the depressor and extensor, operate approximately in phase, while the abductor works with the former group in the case of progression and with the latter group in the case of retreat, the reciprocal holding for the adductor. There are other minor

differences, but this is the most conspicuous one. Incidentally, this alternative association of the shoulder muscles with either one or the other elbow muscle group is a good illustration of the diversity of muscle combinations possible on level O.

Now, if the myochronogram of retreat is projected into the musculature of an animal with reversed forelegs, a movement results such as the one illustrated in the lower half of the center strip of figure 6. As one can see, the effect is that the body is being pushed forward. This the animals have actually been seen to do in the face of a repelling stimulus, and, biologically speaking, the result is as absurd, if not even more so, as in the case of forward locomotion: In their attempts to recede, they bring themselves closer and closer to the stimulus which they tend to avoid. For us the observation proves that even reversed fore limbs can efficiently contribute to forward locomotion of the body if only their muscles are activated in a time pattern appropriate for the purpose. In purely anatomical regards, the reversed fore legs are, therefore, as adequate for forward as for backward motion; hence their persistent failure to cooperate in the total locomotion of the body cannot be ascribed to mechanical incompetence.

A comparison of the myochronograms of ambulation and retreat at the same time permits us to define precisely what changes in the pattern of locomotion would have been necessary in order that the animals with reversed fore limbs might have learned to employ their limbs more judiciously. There are two ways in which the functional incongruity between the normal hind limbs and the reversed fore limbs could have been removed. One would have been to make the adductor and abductor phases trade places in the central time score of progression at the fore limb level, and the other would have been to combine the hind limb fraction of the time score of progression with the fore limb fraction of the time score of retreat. Both changes would have led to essentially the same net results, namely, a transformation at the fore limb level exclusively of the myochronogram of figure 5 into the myochronogram of figure 6. This would have restored harmony between the fore and hind limbs in that it would have enabled the fore limbs, too, to take part in body propulsion (lower row of

fig. 6). In other words, it would not even have been necessary to rebuild the whole locomotor pattern *de novo*. Most of it could have been left unaltered, with a simple shift of the abductor innervation from the elevator-flexor phase to the depressor-extensor phase, and conversely, of the adductor from the depressor-extensor phase into the elevator-flexor phase. This would have involved the time schedule of one muscle pair only. Or the retreat pattern might have been divided into its fore limb and hind limb parts, and the fore limb part alone substituted for the part normally assigned to the fore limbs in progression.

These would seem to be relatively minor changes, and if the amphibian central nervous system had any tendency and power to take into account and to repair inadequate peripheral results, the emergency of the reversed fore limbs should have proved to be a minor problem. As it is, however, it proved to be insurmountable, and neither were corrections effected nor any tendencies at correction, however abortive, ever observed.

The conclusions to be drawn from the reported results are the following: *The chronological scores*, according to which muscles are called into action when a limb is supposed to move *are rigidly fixed*. The centers contain a definite repertoire of such fixed and discrete scores; for instance, one for ambulation, one for retreat, one for swimming, one for righting, one for turning, and so forth, each of which can be displayed only as a whole or not at all. The nervous system cannot recombine for simultaneous execution parts of one score with parts of another score, nor can it alter the sequence and associations among the individual muscles within a given score. In other words, coordination patterns from level S down are ingrained in the centers and are not "effect-determined."

Basic coordination is thus revealed to deal exclusively with the central representatives of muscles, regardless of what effects these dealings will entail. So far as the basic scores are concerned, the muscles might be non-existent. Amphibian coordination operates "blindly," reeling off available central scores evoked by the stimulus situation. In fact, it can be predicted that they would con-

tinue to do so even after the interruption of all motor nerves, or the amputation of all limbs. If it were technically feasible to dissect each muscle free, fully protecting its nerve supply, and then to attach them individually to writing levers,⁹ we should expect to obtain a myochronogram which would in its major lines coincide with the myochronogram reconstructed from the muscle play in a smoothly moving normal limb with all muscles in place. In fact, the result should not be essentially different if we cut all nerves and registered oscillographically the activity of all central stumps. Pieced together, the records should again present the myochronogram of one definite movement or another. This would be true not only of type reflexes, for which our statement is not likely to be questioned because it refers to a standard practice in reflex registration, but also for the much more complex and highly coordinated movements which form the locomotor repertoire of the species.

It will be noted that in this description a possible determining influence of sensory innervation has been completely left out of consideration. This is fully justified by the facts. While a more detailed discussion of this problem will be presented below, we may already in this place point to the fact that in amphibians the basic patterns of coordination are not disturbed by the radical removal of sensory innervation from the muscles executing those patterns.

THE CENTRAL SELF-DIFFERENTIATION OF COORDINATION PATTERNS

Reversed locomotion after pre-functional limb reversal

All experiments thus far reported were done in animals which had already exercised locomotor function for some time prior to the operation, i.e., during a period in which they had been anatomically normal. Thus, while the results have definitely shown that coordination patterns, once developed, remain firmly ingrained in the centers, they have not eliminated the possibility of a constructive influence of experience on these patterns during the embryonic and early larval phase.

⁹ This feat was once actually attempted by Warren Lombard, but with little practical success.

Coghill (1929) has emphasized the autonomy of the early motile patterns of the amphibian embryo. His view is corroborated by the fact that embryos reared under narcosis develop the ability for coordinated swimming (*Harrison*, 1904; *Carmichael*, 1926; *Matthews and Detwiler*, 1926). However, it cannot be overlooked that none of these observations have been extended beyond the early phases, in which motility is confined to orderly mass movements of the trunk, much simpler and less differentiated than the limb movements with which we are concerned. One might, therefore, concede autonomy of origin to the former and still deny it for the latter. This has, in fact, been done.

Some experiments were reported which seemed to indicate that the development of coordinated limb movements depends upon the presence of sensory innervation in the limbs (*Nicholas and Barron*, 1935; *Detwiler and Vandyke*, 1934; *Chase*, 1940). This could be interpreted to mean that the central patterns of motor coordination are gradually built up in the larva under the guidance of sensory control. As we shall explain below, this interpretation cannot be accepted. Nevertheless, one must give credit to these experiments for having called attention to the problem of the origin of coordination in the embryo and for having raised it above the purely verbalistic treatment. No further pertinent experiments on this vital problem were at hand.

Under the circumstances, it seemed promising to adapt the assay method of the transplanted muscle to the study of the developmental phase in the same sense in which it had been applied previously to the post-developmental phase.

Detwiler (1925) had already shown that supernumerary limbs, developed from transplanted embryonic limb buds, exhibit homologous response, just as do limbs transplanted in the functional stage. This demonstrates that muscles which have developed from the start in abnormal positions make just as reliable indicators of myotypic response as do muscles which have fully differentiated in their normal location. As for the origin of central coordination, these experiments were, however, inconclusive since the animals had retained their full complement of four normal legs, which one could assume to have furnished to the central

nervous system the clues which it needs in organizing its motor patterns.

A more crucial situation could be created by exchanging the right and left fore limbs in the stage of buds prior to the onset of function. This operation would produce animals which could never have had experience with normal limbs, but which, according to what was said above, might very well have learned to use the reversed limbs to good advantage, provided the young centers were plastic enough and not occupied by inherited self-differentiated patterns.

The method of transplanting limb buds in amphibian embryos has been carried to such perfection by *Harrison* and his school that the operation meets with no technical difficulty. If the transplantation is carried out after a certain critical stage, the limbs differentiate true to their origin; that is, a left limb bud on the right side will form a left limb. There is one after-effect of the operation, however, which tends to defeat our purpose. As *Harrison* (1918, 1921), and later particularly *Swett* (1937), have shown, the transplantation of a limb bud which is in disharmony with its surroundings on the body is usually followed not only by the differentiation of the transplant, but also by the production on the part of the body of an accessory limb, which then usually is a mirror image of the transplant. Therefore, upon transplanting a limb bud from one side of the body to the other, we would obtain a reversed limb, as intended; but, in addition a secondary limb would sprout out which, being a mirror image of the (reversed) graft, would be a duplicate of the removed original limb. Hence, again an undesirable normal limb would force itself into the picture. In order to evade this difficulty, it seemed indicated to transplant the limbs at a fairly late stage of differentiation, because the ability to suppress local regeneration of extra appendages increases with the age and size of the limb graft. Even so only a small number of cases were obtained in which the operation was wholly successful.

The species used for the experiment was *Amblystoma punctatum*. The eggs were reared in the laboratory. All operations were done in chloretone anaesthesia, the animals being immobilized in a

strong solution (saturated solution diluted in a ratio of 1:8) and then kept for several hours in a solution diluted 1:24. All host animals were chosen at stages in which the fore limbs had begun to show signs of differentiation, but had not yet attained motility. The larvae ranged in age between *Harrison's* stages 39 and 43. In these stages, the fore limbs project from the body wall as rigid, cylindrical, and immobile rods with the contour of the hand plate and the elbow just beginning to appear. The host limbs were extirpated, including as much of the shoulder girdle and shoulder muscles as possible. They were then replaced by limbs of the opposite side taken from donors of slightly older stages. In some cases the donor limbs had already been functional when they were transferred to the younger non-functional host. In removing a future transplant from its donor, a good-sized disk of shoulder girdle and shoulder musculature was taken along with the free extremity.

The transplants were adjusted at the vacated sites of the extirpated host limbs, care being taken to maintain their original dorso-ventral orientation in order to enable them after differentiation to apply their plantar surfaces to the ground. All transplants were at the time of operation far beyond the stage of definitive determination, so that one could be sure that they would continue after transplantation to self-differentiate in all major respects as if they had been left in their original places, that is, with an anatomy exactly the reverse of that fitting the location.

The further differentiation of the transplants occurred as expected. A right fore limb developed at the left flank, and a left limb at the right flank, flexor sides facing posteriorly instead of anteriorly. However, in many cases the transplant had not been successful in suppressing the regeneration of a secondary limb from the host body, and consequently one, and sometimes even two, additional limbs arose near the base of the graft. Fortunately, these accessory regenerates did not affect the conclusiveness of these cases for our purpose, for the following reason. Since the transplants had a head start over the regenerates of several weeks, they became functional and exhibited well coordinated movements long before the regenerates attained motility. Thus they can be

counted along with those other cases in which regeneration had failed to occur.

The operation was successful in twenty-five cases. Thirteen of these died prematurely or had to be discarded for other reasons. The remaining twelve animals with bilateral operations were carried through and form the basis of our further discussion.



FIG. 7. *a*, Salamander Larva With Reversed Fore Limbs (right limb on left side and left limb on right side) developed after the pre-functional limb rudiments of the host (stage 43) had been replaced by limbs of the opposite sides from an older donor (stage 45). Five months after operation. (Gills have been removed for a better view of the limbs.) Note that extensor side of the fore limbs faces headward (compare inset of figure 5). *b*, normal control animal (gills removed).

Only in one of these cases, illustrated in figure 7, had regeneration failed to occur on both sides, so that this animal had no other fore limbs except the two reversed transplants. In 4 cases the transplant of one side remained single, whereas the transplant of the opposite side was later joined by either one (fig. 8) or

two regenerates. In the 7 remaining cases regenerates appeared on both sides, leaving both transplants single only during the earlier stages.

The transplants received innervation from the severed brachial plexus of the host, but since the conditions under which such re-innervation occurs have been amply studied (*P. Weiss*, 1937 b),



FIG. 8. SALAMANDER LARVA DEVELOPED AFTER OPERATION AS IN FIGURE 7A

Behind the reversed transplant on the right flank, the host has regenerated a right limb. (Donor: stage 46; host: stage 43; fixed 5 months p. op.)

a detailed anatomical study of the present cases was omitted. The earliest functional contractions were seen in the transplanted limbs about two weeks after the transplantation, which interval may then be taken to be sufficient for successful regeneration of the nerves. Even in older larvae two weeks are sometimes adequate for the re-innervation of transplanted limbs.

The observations on the function of the reversed limbs were so uniform that they can be summarized in a single sentence:

From the very first stages of motility, the limbs moved in reverse. This is as true of the stages of incipient function, in which the limb moves in association with the trunk (Coghill, 1933), as it is of the later stages, during which the limb exhibits independent and more differentiated action. In some cases the excursions of the shoulder joint were of reduced extent. The movements of the elbow, the wrist, and the fingers, however, were always smoothly executed and of normal amplitude. The restriction of the shoulder movement is to be ascribed to slight anatomical deformities of the transplanted shoulder girdles, and in a few cases may also be due to some remnants of the host shoulder muscles counteracting those of the transplants. The shoulder movements were more seriously affected in those cases in which a regenerate had developed and become fused to the base of the transplant.

For the rest, however, the transplants exhibited precisely the same form of coordination which we described above for fore limbs reversed in the functional stage. As in those earlier cases, the same combination of muscles was active at any given moment in the reversed limb which would have been active in the same phase of movement in a normal unreversed limb innervated from the same central district. The effect was again a persistent effort of the fore limbs to move the body backwards whenever the rest of the animal attempted to progress. Some of the animals were kept up to six months after the operation, but they never showed any sign of modification of this peculiar locomotor coordination. Of course, no such adaptation was to be expected from what the earlier experiments on older larvae had taught us. Besides, the point to be stressed here is not that the animals have retained their reversed locomotion, but that they had developed it in the first place.

In those specimens in which one or two regenerates arose next to the transplant, the resulting limb duplet or triplet exhibited again "homologous response." Since cases of this kind have been amply described in previous publications, we merely mention them without further comment.

The inherited motor repertoire

Let us now briefly consider the implications of these experiments. The reversed forelimbs have from the beginning functioned in a manner which precluded their useful participation in the locomotion of the body. All the points discussed in connection with limb reversal in older larvae hold for these younger stages as well. Just as in the older cases, the objection that the reversed movements might have been enforced by mechanical limitations is invalidated by the fact that when the animals retreat the reversed limbs do exert satisfactory forward traction; but then, of course, the hind limbs march backwards. Combination of the retreat pattern for the fore limbs with the advance pattern for the hind limbs would have straightened out matters, as we explained above. But the early developing nervous system was no better at solving the problem than were the more mature centers of the older larvae.

These animals have never in their lives experienced normal locomotion. Nevertheless, they have developed the same typical central patterns of locomotor coordination which make an anatomically normal individual walk. In other words, the centers have come to "walk" correctly, even if the limbs have not. Surely, therefore, the experience of actual walking can play no part in molding in the centers those patterns of coordination through which walking is effected, as the heuristic theories would have it. Nor does it seem to make any difference for these patterns whether they produce effective walking or—as in the case of our experiments—some wholly incongruous performance, which is quite contrary to the systemic theory, according to which the total effect counts. Consequently, *neither a heuristic nor a systemic explanation of the origin of coordination can be harmonized with the facts.* Neither experience gained through trial and error in performance, nor autonomous regulative powers of the nervous system can subordinate the timing of the muscles to the needs of the body as a whole.

Of far greater significance, however, than this negative statement is the positive information contained in the experiments,

namely, the proof, firstly, that the centers develop and elaborate the patterns of coordination by pure *self-differentiation* and, secondly, that this differentiation occurs in the *myotypic code*.

It must be kept in mind that our experiments have altered the normal anatomical relations not only between muscles and body, but also between spinal cord and muscles: In removing the host limb to make room for the transplant, the young limb nerves were interrupted, which must have abolished any original point-to-point correlation between spinal cord and musculature, if ever such a point-to-point correlation existed. Then, the transplant was inserted, and the severed nerves grew into it. The facts of nerve regeneration are such that, statistically speaking, these nerve fibers growing out for a second time must have come to land on entirely different muscles from where they had ended before. Thus, each muscle of the transplant is finally connected with an entirely different constellation of ganglion cells than is the corresponding muscle of an unoperated limb. That this does not confuse the muscular control, is to be credited to the principle of myotypic response.

Any more concrete idea of how this may occur, depends on what particular explanation of the mechanism of myotypic response one favors. As we have said before, this is still largely a matter of conjecture. If we accept, for instance, the suggestion that each muscle after its own differentiation "modulates" its motor nerve fibers, this retrograde action would project the various muscular specificities into the motor nuclei and there set up a true representation of the effector system, independent of the peripheral nerve topography. Thus, even if the nerves are distributed over the limb at random, the ganglion cells of the motor columns of the spinal limb segments will at the end of modulation consist of approximately two dozen different varieties, each type corresponding to a particular muscle. How closely the ganglion cells belonging to an individual muscle are assembled, is still a controversial question. But it is obvious that even if a strict topographical projection of the musculature in the cord existed in a normal animal, it would be abolished in the course of peripheral nerve regeneration. Consequently, in our experimental animals

the various motor cells of the two dozen varieties must have been profusely intermingled.

These cells now are the ultimate executives of the centers. In dealing with them, the centers seem to have no additional means of discerning just how they are connected with their muscles, how the muscles are connected with the skeleton, what kind of movements the contractions of these muscles will produce, and finally, what the outcome of these movements will be for the body as a whole. Thus, when the central score requires the activation of, for example, the pectoral muscle, which in a normal animal adducts the arm, the pectoral-specific "call" is emitted within the limb cord and is responded to by the pectoral-modulated ganglion cells, even though the pectoral muscle, as is the case in the reversed legs, may lie at the anterior instead of posterior border of the limb, abducting instead of adducting the upper arm, and thereby producing a backward instead of a forward swing of the body. Moreover, the pectoral-modulated cells are likely to be quite a different group from what would have been the pectoral center in the normal animal.

While these facts are merely a confirmation of the general validity of the principle of myotypic response, the new point brought out by the experiments concerns not so much the *response* of the motor units as their *selective activation* according to firmly set time patterns, self-constructed by the centers in forward reference to, but without the constructive aid of, actual function.

How are we to visualize these patterns? It must be admitted that the prospect of identifying their material basis and dynamic properties is not yet very bright. It seems that all we can do at present, is to reconstruct them from their manifestations. We know that when we provide them with a system of detector muscles, they will produce a definite myochronogram for each particular act, such as exemplified in figures 4, 5, and 6. The myochronogram, therefore, may be regarded as a peripheral projection of the time schedule according to which the various modulated ganglion cell groups are being activated. If we could let all motor ganglion cells register their phases of activity in separate

tracings, this would give us a direct record of their time schedule—a “*neurochronogram*.” The neurochronogram, in turn, is only an expression of the time order in which the principle which activates the motor cells becomes effective, a record of what we have called the “central score.” This is as far as our factual knowledge will take us, and unless we want to enter the field of hypothesis, the nature and localization of the agents in back of it all must at present remain unaccounted for. Even so, however, we can make certain definite statements about their reality, characteristics, development, behavior, and effects.

(1) The central score remains constant amidst the reported experimental changes forced upon the periphery. This stability of organization, therefore, justifies the assumption of an underlying central state or process of correspondingly definite organization.

(2) The organization of the scores on level O (coordination within a limb; “intra-appendicular” coordination) is stable only for a given motor act. The fore limb score for ambulation, for instance, provides for the combination of the adductor, depressor and extensor in one team, and of the abductor, elevator and flexor in the other. The score of the same center, however, for the act of retreating combines the abductor, depressor and extensor in one phase, and the adductor, elevator and flexor in the other.

(3) The intra-appendicular scores on level O are indissoluble in their composition within any given act. Even urgent biological necessity cannot modify the chronological patterns exemplified under (2).

(4) The scores on level S (coordination among limbs; “inter-appendicular” coordination) for a given act (ambulation; retreat; etc.) operate through the subordinated intra-appendicular scores provided for those acts according to (2). But the time relations among the individual limb actions are more variable than the time relations of muscle actions within the limb (see p. 36).

(5) In spite of greater temporal elasticity, the combination of partial scores of level O executing interappendicular coordination of level S is indissoluble within a given act. Fore limb scores of retreat cannot be combined with hind limb scores of progression even under the pressure of biological necessity.

(6) The scores provided for the different basic biological acts appear to be discrete entities with no intergradations. Each one represents a ready mechanism to execute one vital function with great adequacy, provided the animal is equipped with just the kind of anatomical apparatus for which the score is designed, including proper nerve modulation.

(7) The basic scores develop by self-differentiation within the centers, molded by the pre-functional agents of embryonic differentiation. Not all of them develop at the same time, and many are not completed until late in post-embryonic life. Some are permanently in operation (e. g., respiratory movements), others may remain latent under ordinary circumstances. The totality of preformed scores owned by a species may be called its "*motor repertoire*."

(8) Since different scores are executed by the same muscles and ganglion cells, only in different assortments and sequences, it becomes impossible to view them in terms of rigidly established neurone connections after the fashion discussed on page 8. Consequently, the definiteness and constancy of their structure must be based upon definite dynamic properties (time parameters; chemical affinities; or the like) of the central agents, rather than on unique morphological connections.

According to these points, the amphibian central nervous system self-differentiates a definite and limited repertoire of discrete and strictly circumscribed scores upon which the animal must draw for all its performances. Animals of the type here discussed have never shown any ability of adding to this inherited repertoire in their later life by experience. *The elementary mechanisms of coordination are inherent in the centers, and the units of coordinated behavior are integrated complexes of the kind reflected in a myo-chronogram*, rather than individual neurone activities. This is the basic fact which all theories of coordination will have to keep in view, and compatibility with which will be their test.

Muscle center vs. impulse pattern

In order not to vitiate the factual content of the experiments by speculative admixtures, we have refrained from trying to

translate into more concrete terms the essentially formal conclusions to which our experiments have led. We have derived from our observations as much insight into the origin of coordination as the method employed has been able to yield. But our formulations have not gone beyond evaluating the phenomena and furnishing a definite conceptual frame which any theory of coordination will have to fit. On the negative side this has led to the elimination of certain theories of coordination favored in the past, for the reason that they are in conflict with the facts. On the positive side, however, we have not been able to establish anything more concrete than the fact that the central agents to which the chronological scores refer, have real existence, strong individuality, and develop their properties, as well as maintain their identity, in the face of all kinds of peripheral changes. To study their nature, remains an object for future research. Our experiments have merely put this objective into clear view. As a first move toward this goal, a few speculative comments will here be added. But in offering them, it should be understood that, whatever criticism they may arouse, would not shake the factual content of the experiments thus far discussed.

The problem is plain: The motor columns consist of a fixed number of nerve cells, modulated into a few dozen varieties, and susceptible to activation by agents of corresponding specificity. What is it, then, that makes those agents appear in such standardized chronological order as to yield the stereotyped pattern of activation represented by the "score?" One is tempted to think of a piano in which keys are operated in definite combinations and sequences.¹⁰ However, in a piano, any combination of keys can be played. Possibly in the amphibian nervous system, too, any combination of motor cells *can* be activated; but the fact is that only certain definite ones actually *are* activated. Thus, if the piano were to serve at all as an analogy of the central apparatus, it would have to be the automatic kind of piano with its limited repertoire. An electric piano is made to give off a definite tune

¹⁰ Except that in the piano the position of the key in the scale is a clue to the effect (tone) it will produce, whereas in the case of the motor centers the clues consist of the specific moduli acquired by the cells in the course of modulation.

by running a properly cut out stencil over contact points which release a corresponding set of keys. The harmony of the tune is preformed in the pattern of the stencil. Similarly, we understand the operation of moving electric light signs as the result of a properly shaped contact brush sweeping over a field of contacts, each of which has a one-to-one connection with a single light bulb.

But this analogy does not stand up under closer inspection. For one thing, there is no constant topographical correspondence between the muscles and their central representatives, as there is between keys and contacts. Even if individual muscles should be proven to possess each a localized nuclear representation in the motor part of the spinal cord under normal conditions—a fact which, as we mentioned before, is still disputed (see *van Rynberk*, 1908)—random nerve regeneration would upset the scheme without consequence for the peripheral coordination of the muscles. For we have seen that experiments which enforce random connections do not upset the strict functional correspondence between center and muscle. In contrast, an automatic piano with the wires between contacts and keys crossed gives off a tuneless dissonance. Thus, we had better not follow the analogy any further.

In conversations bearing on the homologous response of supernumerary limbs, it has often been suggested that the motor cells newly connected with a certain kind of muscle might through a selective union of dendritic processes have managed to join with the motor cells connected with the synonymous normal muscle, thus tuning themselves in on all calls going to the normal muscle, much in the way of a two-party telephone line. Nothing was said about how the coordination of the normal muscle centers was achieved, but this, of course, was tacitly assumed to be simply a matter of one or the other of those theories (switch-board; heuristic; systemic) currently in favor as explanations of coordination. However objectionable for other reasons, this view becomes untenable in view of the animals with reversed fore limbs. For in these cases, whatever may have normally passed for a “normal muscle center” with topographical identity, is now dissipated at random over the whole extent of the limb segment. Surely, in a physiological sense, one could still speak of “muscle

centers" including under this term the totality of ganglion cells modulated to the tune of a given muscle. Dispersed as they are, however, no topographical sense could be connected with this designation.

It is fully conceivable that motoneurons of the same tune might become knit together into functional unions through their widely ramified dendritic processes. In other words, it is quite possible, as I have suggested previously, that the process of modulation extends farther centrally and thus permits the innumerable cell processes of the neuropil to identify each other. However, this does not really bear on the essential problem of coordination, because the question still remains: why do these muscle centers always fall into certain definite patterns and chronological sequences of action, and only into these patterns, instead of being plastically associable, as has been the view of most theories?

One might try to solve the difficulty, or at least remove it by one step, by assuming that the real muscle centers are not composed of the motor root cells, i.e., the "final common paths" of *Sherrington*, but lie farther centrally in the internuncial gray from which the motor neurons derive their excitation. This assumption would save the concept of a separate localized center for each muscle; one would have to conceive of them as biochemically differentiated neurone pools within the central gray with which the efferent neurons would effect dendritic contact selectively, each one with the appropriate focus according to its modulation. These central stations, one neurone removed from the motor effector units, might then be treated according to the well known schemata of reflexology.

This view would deserve more serious consideration if the seat of basic muscular integration were to be found in the brain at supraspinal levels, where a considerable amount of gray matter is still without physiological assignment. But since we know that a spinal amphibian exhibits well coordinated movements of its extremities (last reported by *Gray* and *Lissmann* 1940b; "homologous response" in spinal animal: *P. Weiss*, 1937a), it is the spinal cord alone to which we have to look for the mechanism of basic coordination. The histology of the amphibian spinal cord,

however, discourages emphatically the attempt of identifying anything like discrete muscle centers in its internuntial gray.

Furthermore, the question as to why definite combinations of muscles, and only a very limited choice of such combinations, comes into real operation, would still remain as obscure as before. Recourse to the assumption of stereotyped anatomical connections between definite muscle representatives on the one hand and higher centers or sensory nuclei on the other is of no avail. As we have pointed out earlier, coordination on the basis of built-in anatomical connections has been demonstrated in certain cases and might possibly be passable as an explanation of the coordination of hinge joints, the paradigm of conventional reflexological schemata. It is irreconcilable, however, with the existence of multiple-choice reactions, as exemplified by forward and backward locomotion, in both of which depressor and extensor act essentially in phase, while the adductor may be engaged either in their phase or in the antagonistic phase, and so may the abductor. Obviously, the morphological connections are equally suited for either form of association, and, hence, the choice must depend on physiological factors. Other patterns may show the elevator and flexor out of phase, and the depressor dissociated from the extensor; hence, their association in progression and retreat cannot be anatomically fixed. In general, different patterns show the muscles in different associations. Yet, how these functional associations are brought about, and why they are as strict as they are, is beyond our present knowledge. The heuristic theories had an answer for it, the systemic theories at least a claim to an answer. With both these now eliminated, so far as *basic* coordination is concerned, we must start our search anew. It will probably be helpful in this search not to follow the ruts of the now discredited scheme of rigid fiber connections as the basis of coordination, and turn from a topographic-geometric concept to a more dynamic concept in which a "muscle center" would be a *specific central state of activity* characteristic of a given muscle.

Of course, if we concede nothing more to the centers than what is found in the periphery, the task is hopeless, just as all the phenomena we have described would become incomprehensible.

However, if we accord to the centers an unbiased investigation, we might some day discover that the "excitatory state" of *Sherrington*, for instance, may not be after all such a trivial and colorless condition as electrotonus, or the concentration of a chemical substance, but that it may assume a variety of differentiated states in which *specific biochemical configurations* or *specific electric parameters* would appear as keys selectively unlocking for the discharge of excitation peripheral channels of correspondingly specific configuration. Yet, it seems quite futile at the present state of our knowledge to indulge in these vague prophecies. And we wish to repeat that their rejection in toto would not in any way reflect on the experimental facts and conclusions presented earlier in the paper.

THE ROLE OF SENSORY CONTROL

Motility after de-afferentation

The inadequacy of both heuristic and reflexological interpretations of coordination is further underscored by the fact that sensory excitations play a constructive part neither in the origin nor in the maintenance of basic coordination patterns in amphibians.

Ever since *Exner* (1894) advanced his principle of "sensomotility," the importance of sensory control in the execution of movements has been staunchly defended. Observations on motor defects following sensory disturbances in mammals, clinical observations of similar nature, the importance of sensation in the re-education of the muscular coordination in patients with transposed muscles, and finally the emphasis placed on the afferent input as the key to the motor response in all reflexological interpretations of coordination, have contributed to the popular notion that motor coordination would break down in the absence of sensory control. Extrapolated into the embryonic phase, this notion would suggest that without sensory control motor coordination could not be established, either.

While this is not the place to give the whole problem as thorough a consideration as it would deserve, its relation to our subject matter is close enough to require some comment. Heuristic,

systemic and reflexological theories of coordination have considered it vital, each for a different reason, to ascribe to sensory innervation the role of a determining agent in the development and maintenance of coordination. According to the heuristic view, the sensory messages provide the centers with the necessary reports of the success or failure of a tried muscular effort, so that the effort may be either repeated or abandoned. According to the systemic view, the total sensory input creates a definitely shaped central state which, in turn, determines the character of the total response. According to the reflexological view, each sensory channel opens into its own peculiar system of central communications, the efferent ramifications of which determine the pattern of the response. But all three are agreed that the sensory influx is indispensable for coordinated motility. So far as the *basic* patterns of coordination are concerned, this is a wholly untenable thesis.

The whole matter is one about which there seems to be still as much divergence of opinion as there ever has been. The controversy rests not so much on discrepancies of experimental results as on divergent interpretations, loose or mistaken formulations, and a heavy dose of anthropocentric thinking of the kind referred to on page 20. In order to avoid further confusion, let us be explicit about the issue: It is not whether sensory functions are "important" or not, and whether or not the elimination of sensory function "affects" the motor behavior of an animal, because nobody would hesitate to answer both these questions in the affirmative. The point with which we are solely concerned in the present connection is whether the *patterns* of coordination, i.e., the organized combinations of muscles into definite chronological scores, owe their *organization* to any effects exerted through afferent nerves. We further confine the issue to the *basic* patterns of locomotion, excluding the finer adjustments, and discuss the problem, for the time being, only as it applies to the *amphibians*.

Within these limits it can be safely stated that sensory control plays no *constructive* part in the development and maintenance of the patterns of motor coordination. To qualify this statement, we may first point to evidence coming from experimental

de-afferentation of limbs. The experimental results are rather convincing because most authors, even though favoring diametrically opposed interpretations, are in essential agreement about the underlying facts. It has been observed by *Brown-Séguard* (1850), *H. E. Hering* (1896), and *Bickel* (1897), that frogs continue to use their legs in essentially unimpaired coordination after the dorsal roots of the limb segments have been transected. These observations were confirmed and amplified by more recent work on the toad and on salamanders (*P. Weiss*, 1936 a; 1937 c). Still more recent repetitions of the experiments by *Chase* (1940) and by *Gray* (1939) have added no data that would conflict with the earlier findings. According to all these findings, a limb completely deprived of sensory innervation continues to take part in locomotion, such as jumping, walking, and swimming, as well as in postural and other reactions, such as righting, turning, wiping.

These facts prove that the basic patterns of coordination, at least up to level O, are not acutely controlled by sensory impulses from the peripheral organs through which they are effected. Just as we have seen the inherited central motor patterns to survive in the face of *altered* peripheral conditions, so we find them to continue under a complete *lack* of clues concerning their peripheral effects. These same experiments disprove the theory of the rhythmic gait as a series of alternating stretch reflexes. While stretch reflexes may, and probably do, re-inforce and smooth out various phases of the locomotor acts, they are not instrumental, and not even indispensable, in producing coordinated locomotion.

The undulating swimming movements of the trunk of an amphibian are as little the product of a self-perpetuating chain reflex as is the alternating gait of an individual limb. Experiments by *von Holst* (1935) have already demonstrated that the swimming mechanism in fishes does not consist of alternating stretch reflexes of symmetrical segments of the trunk musculature, as had formerly been suggested. After cutting most of the sensory roots, he observed no major impairment of the swimming rhythm, which led him to the conclusion that swimming is effected by autonomous intracentral waves of excitation passing down the

length of the spinal cord with a definite phase shift between the left and the right sides. Since *Gray* (1939, p. 42) has questioned the conclusiveness of *von Holst's* results, because a few dorsal roots had been left intact in the operated fishes, I recently repeated the experiments with amphibians, following a more radical procedure (*P. Weiss*, 1941). In frog tadpoles, *all* spinal dorsal roots were cut on both sides way down into the tail base, and the remaining part of the tail, where de-afferentation is less well controllable, was amputated. Swimming coordination of these animals was as good as that of normal animals.

In view of the existing evidence, which, as one can plainly see, rests on a solid and consistent experimental foundation, it would be hardly necessary to dwell on these points any longer, were it not for the fact that *Gray* (1939) and *Gray and Lissman* (1940a) have recently come out strongly in the defense of the reflex chain theory of ambulation. In point of fact, they confirm that amphibians with one, two, three, or even all four limbs de-afferented are still capable of performing all the typical acts of locomotion. Their description coincides in all major points with the earlier report on the subject by *P. Weiss* (1936a): There is agreement that with increasing extent of de-afferentation, (a) the *spontaneity* of the animals, i.e., reactivity on the level W, decreases; (b) the effects of loss of postural control and possibly tone become increasingly conspicuous in the form of *cruder* and less well balanced movements; neither of which defects, however, can obscure the fact that the *typical coordination patterns* of levels O and S, the former integrating muscles into limb movements and the latter integrating different limbs among one another, have not been abolished and cannot, therefore, be under sensory guidance.

We prefer to place the emphasis on the persistence of the patterns as such, regardless of whether or not their execution is equally easy and equally smooth. That the sensory channel may be necessary in order to start the centers off, is an entirely different problem. It requires a switch to start an electric plant going, and fuel must be fed into an engine to make it operate. In a normal animal, aside from respiratory and highest brain functions, some stimulus is obviously necessary to set off a reac-

tion, but that does not mean that the pattern of the reaction is likewise determined by the input, as little as the distribution of the electric power or the mechanism of the machine are determined by the switch or the fuel. Thus, if *Gray and Lissman* state that ambulation¹¹ is impossible unless *some* sensory fibers, even though few in number, are left intact *somewhere* in the trunk region, one will agree that even if this statement were to be taken at full face value,¹² it would only confirm the *autonomy* of the central patterns of coordination. For if the stimulation of *any* kind of nerve entails such a highly organized performance as ambulation, and invariably the same standard pattern, no matter which nerve was stimulated, this should satisfy the authors that the *character* of the response cannot have been determined by the stimulated nerve. This is merely a matter of logic.

In a subsequent paper, *Gray and Lissman* (1940b) describe interesting observations on locomotor patterns obtainable in spinal amphibians. In line with what is well known to everybody who has occupied himself with this field, they describe how a stretch reflex may set off a partial pattern of locomotion, that is, a coordinated limb reaction on the level O, frequently entailing a spinal reaction on the level S, engaging all four limbs. How the authors can be so inconsistent as to suggest from this observation that the act of ambulation actually amounts to nothing more than a series of stretch reflexes elicited from the legs, after they have themselves in the immediately preceding paper demonstrated the persistence of the ambulating pattern, even after all four limbs had been de-afferented, is difficult to understand.

Moreover, if the authors had paid attention to the locomotion of our animals with reversed fore limbs, as described and depicted in the 1936 paper, they would have realized that these specimens carry the whole stretch-reflex-chain idea ad absurdum. For in these animals diagonal limbs (e.g., left fore limb and right hind limb) move in opposite directions, rather than in parallel as do

¹¹ The authors are no longer equally positive about other types of locomotion (1940a, page 232-233).

¹² Coordinated locomotor movements have actually been observed in toads in which the whole length of the spinal cord was de-afferented (*P. Weiss*, 1936a).

diagonal limbs in a normal walking animal (see p. 46). If muscle stretch due to the drag of the advancing body were responsible for the functional linkage between the four limbs, it should have operated in the reversed fore limbs in the same sense as in normal limbs, resulting in effective progressive ambulation, which did not happen. Or if the reversed fore limb movements had set the pace, the backward drag imposed upon the hind limbs should then have made the latter cooperate in a coordinated backward movement; which did not happen, either. What did happen, was that animals possessing reversed fore limbs and normal hind limbs remained stationary, with each one of the four legs going through the motions of what would be its part in typical ambulation, but without the body as a whole moving at all. This should have cautioned the authors against vesting the simple stretch reflex with imaginary pattern-determining powers. In fact, a mere consideration of the normal retreat pattern, to which the authors give no thought, but in which, as we have seen, the abductor-adductor associations with the flexor-extensor phases are exactly the reverse from what they are in forward ambulation, would have upset their scheme, the more so as they themselves deny the existence of stretch reflexes in the adductor groups, which are the ones that would be subject to passive stretch in backward locomotion. On the whole, if the losing cause of the reflex chain concept of locomotion needed any further weakening, the inconclusive and contradictory re-interpretation of otherwise sound experiments by *Gray and Lissman* has certainly produced it.

While the suggestion of a *constructive*, that is, pattern-forming, role of the sensory influx must be emphatically rejected, the *regulative* influence of sensory innervation is duly appreciated. While the major characteristics of the locomotor pattern are laid down as autonomous functions of the centers, the minor details of their execution are undeniably under peripheral control. Failure to keep the two issues separated, confuses the discussion, and many misunderstandings of the past can be referred to this failure. To be sure, the distinction between "major" and "minor" is somewhat arbitrary, but no more so than the decision

in physical and mathematical matters as to where quantities become negligible.

Plainly there are differences between the movements of a fully sensitive and a de-afferented limb, as well as differences in the behavior of the whole animal, which become increasingly greater as the extent of de-afferentation increases. The finer polish through which the movements become smooth and are kept in harmony with the changing topography of the environment disappears, and only the crude basic structure of the main patterns is left. But the difference between the polished behavior of the normal animal and the cruder performance of the de-afferented animal is so much smaller than the difference between the still highly coordinated function of the latter and a disorganized state of random contractions, which would mark the break-down of coordination, that it becomes practically negligible so far as the problem of basic locomotor coordination is concerned. The step from the irregular twitching of an uncontrolled muscle machine to the coordinated activities observed even in de-afferented animals is so immense, when compared with what sensory control has to add in the way of further accomplishment, that our sense of proportions should revolt against the recurrent attempts to give sensory control full credit for the whole achievement.

However, clarification of the whole issue will be greatly aided, if we abandon such inarticulate utterances about sensory control as that it is "of paramount importance," "dominant," "essential," "vital," or, on the other side of the picture, "irrelevant," "practically insignificant," etc., and replace them by precise statements as to what phases of motor activity depend upon the integrity of sensory innervation, in what respect, and to what degree. Such a program would make no sense unless the hierarchical constitution of nervous functions is recognized. But if we admit that sensory influx may have different effects with regard to some levels of nervous activity than with regard to others, we realize the necessity of a more differentiated rating of those effects, than merely as a point on a scale from "unimportant" to "highly important." Without trying to be exhaustive, here is a brief list of known sensory functions as they affect motor behavior.

(1) The afferent influx initiates responses by releasing central discharges of definite pattern.

(2) It conditions the centers for subsequent excitations by residual effects on central excitability and excitatory state. The total afferent influx thus produces a continuously shifting background of central excitability, which explains much of the latitude of the stimulus-response relation.

(3) It decides which response from among the plurality of latent discharge patterns composing the central "repertoire" is actually to go into effect. It also influences direction, intensity, speed and duration of the elicited response.

(4) Afferent proprioceptive impulses control the degree of muscular tone, and hence, maintenance of posture against gravity.

(5) They also contribute to the precision and smoothness of a movement through local stretch reflexes (myotatic reflexes) acting as "governors."

Since each major item of this list can be still further subdivided, it will be realized how complex the effects of sensory influx are, hence, how futile it is simply to assert their bearing on motor functions without further qualification. A toad with de-afferented hind limbs moves perfectly well over rough ground; but when it happens to land from a jump with its limbs contorted, no postural correction will ensue until the next locomotor impulse automatically returns the limbs to their normal position. This exemplifies the kind of disturbances to be ascribed to lack of sensation. Other shortcomings are the cruder dosing of the muscular contractions, exaggeration of movements, abnormalities of the tonic background, etc., none of them serious enough to mask the essential fact that the basic central scores through which locomotor coordination is effected survive the elimination of the sensory influx.

Many statements to the contrary can be traced to obvious sources of error. The most trivial is primary damage done to the motor centers in the course of de-afferentation. When *Moldaver* (1936, p. 457) states that "the transection of the dorsal roots in the toad results in an immediate deep depression of spontaneous motor activity of the de-afferented leg, which presents

the aspects of paresis, or even veritable flaccid paralysis," his result cannot be reconciled with the ample evidence to the contrary, except by assuming traumatic motor damage; for if the de-afferentation is performed with the necessary care, leaving the spinal cord unexposed (*P. Weiss*, 1936a), no such effect is noted, not even immediately after the operation.¹³

Less easy to explain, however, are certain secondary late effects of de-afferentation which seem to hamper insensitive limbs in the execution of movements. In amphibians these effects become distinct several weeks after de-afferentation. While varying in extent among different individuals, their common denominator, as judged from observations of *Brown-Séquard* (1850), *Weiss* (1936a), and *Chase* (1940), seems to be a marked *hypertonicity* of the limb muscles, tending to throw the legs into rigid extension and adduction. Whether this exaggerated tone affects the extensor and adductor muscles selectively, or applies to all muscles, with the extensors and adductors simply dominating in appearance owing to their greater weight, has not been determined. At any rate, while it lasts, this hypertonicity prevents the limb from taking part in locomotion. Yet, one commonly observes that, when the muscles limber up temporarily, normal coordinated movements are resumed (*P. Weiss*, 1936a; *Chase*, 1940), and, reciprocally, that with the onset of a locomotor sequence the hypertonicity may abruptly give way to unhampered movements, only to reappear again during the following resting period.

These observations in themselves demonstrate clearly that we are not dealing with a real deterioration of the patterns of coordination—their integrity has not suffered appreciably—but rather with the development of some new pathological condition which blocks the execution of movements. There is evidence that de-afferentation of an amphibian limb automatically raises the excitability of the corresponding motor segment (*Moldaver*, 1936), thus submitting the motor centers to bombardment by

¹³ It is gratifying to the author that a satisfactory clarification of this point could be reached during a personal visit in 1937 to the laboratory of Dr. *Bremer* in Brussels where Dr. *Moldaver* was working.

impulses that would otherwise have remained below the threshold of effectiveness. The central tone, which normally merely furnishes the background upon which the locomotor innervation is superimposed, may thus produce in the hyperexcitable de-afferented region a state of chronic excitation of such extent that no motor units would be left for the execution of coordinated acts. By engaging all motor pathways, the hypertonic state would deprive the mechanisms of coordinated locomotion of their effectors. This would explain the immediate resumption of coordinated movements upon any temporary subsidence of the tonic phenomenon. One could also imagine that the relentless bombardment of the de-afferented motor segments might in due course of time produce some lasting damage, although there is, at present, no real foundation for such an assumption. But whatever the explanation of this interesting phenomenon of hypertonicity after de-afferentation may turn out to be, the outstanding fact remains that the phenomenon merely supersedes locomotor activity and by no means signifies disintegration of coordination. Consequently, the attempt to advance this phenomenon as indirect evidence of a constructive role of the sensory influx in the formation of patterns of coordination cannot be regarded as warranted.

Development of motility without sensory control

The point is of particular significance in view of the fact that similar observations were made in animals whose sensory system had been damaged in the *embryonic* phase, giving rise to the claim that the integrity of the sensory system is a prerequisite for the differentiation of the motor patterns (*Detwiler and Vandyke, 1934; Chase, 1940*). The experiments in question were done in larval urodeles and consisted of the removal of the dorsal portion of the neural tube, which includes the neural crest rudiments of the spinal ganglia. Such embryos develop into larvae with defects of varying degrees in both spinal ganglia and spinal cord. Functionally, the larva exhibited the full range from undisturbed to completely paralyzed limb motility. Since the functional disturbances could have been due to either sensory or motor

defects, the authors made careful estimates of the relative degrees in which either central component was affected in various cases. As a result of these studies, they have come to the conclusion that the severity of the peripheral functional trouble is correlated with the degree of damage done to the sensory rather than to the motor system.

Whether or not their conclusion is accepted, it is reasonable to assume that at least part of the motor disturbance was due to the underdevelopment of the afferent system. It is noteworthy, however, that this disturbance was more in the nature of an excess than of a deficiency, in that the limbs were afflicted by the same kind of excessive extensor and adductor tone which had been observed in de-afferented adult limbs. Thus the question arises: Did the central motor patterns in these cases remain undeveloped, or did they develop but become chronically overridden by an exaggerated tone occupying the whole available motor pool, in the manner discussed above? The description of some of the operated cases seems to give a clear answer. We read, for instance: "Although both legs tended to hyperextend at rest, all reflex and spontaneous movements were considered to be normal" (*Chase*, 1940, p. 74). Obviously, the mechanism for coordinated reflex and spontaneous movements had not failed to undergo differentiation. It was only covered up by the hypertonicity, which can perhaps be explained in the same way as in the analogous adult cases: hyperexcitability of the motor centers as a result of the chronic weakness of the sensory influx. Moreover, the possibility that the sensory system may exert some trophic influence upon the motor parts of the cord cannot be ignored. One could think of the hypertonicity of the de-afferented limbs as a result of trophic disturbance, but the data are insufficient for any decision. At any rate, so long as we do not acknowledge the hypertonicity as a sign of disintegration of the coordinative mechanism, its further explanation becomes a side issue.

The embryonic experiments just reported have not brought the desired clarification of the issue. If anything, they speak against a constructive part of the sensory control in the development of coordinated locomotor patterns. The main reason why they

were bound to remain inconclusive is that damage was mostly done to both the sensory and the motor systems. In the urodeles, which were used for the experiments, the limbs develop in the embryonic and early larval period so that prefunctional deafferentation is feasible only by removing the whole rudiment of the ganglionic column (neural crest) in bulk. Although this has been done with great skill (*DuShane*, 1938), one has no assurance that the removal has not deprived the differentiating neural tube, if not of some cellular constituents, so at least of some embryogenetic and trophic influences possibly required for its elaboration into a normal spinal cord. This difficulty can be avoided if *anurans* are used instead of urodeles.

The tadpole of the frog develops hind legs not until comparatively late in larval life, at a time when the central nervous system has reached a very advanced stage of differentiation and the animal has been functional for several months. The spinal ganglia and dorsal roots are in their definitive positions and can be easily and neatly extirpated without causing damage to the spinal cord. This was recently done in a number of tadpoles whose limbs had not yet reached the functional stage. Since the loss of the ganglia precluded regeneration of a sensory supply, these limbs grew up with purely motor innervation, which was confirmed by their lack of sensitivity to stimulation. While in certain cases secondary complications, not of concern to us in the present connection, tended to obscure the picture, the general result was that a number of these animals developed coordinated locomotor function in hind limbs from which no sensory message had ever gone to the centers. A full report of these experiments will be presented on a later occasion, but it seemed desirable to refer to them in this place in order to stress that *locomotor coordination can develop by central self-differentiation of the motor centers in the complete absence of sensory control*.

On the whole, we see, therefore, that a close scrutiny of sensomotility has failed to reveal facts that would militate against the theory of the autonomous development of motor patterns by *central self-differentiation*, the primary evidence for which we had adduced from recombination experiments. In this sense, the

results of the recombination and the defect experiment mutually support each other.

COORDINATION IN MAMMALS

If central self-differentiation unguided by peripheral experience is so dominant in the establishment of motor coordination, how is it then possible that the recognition of this fact could so long have remained in doubt? Two reasons may be advanced: First, the lack of adequate experimentation; second, the anthropocentric factor, that is, the tendency, mentioned in the introductory chapter, to interpret animal behavior on the basis of human experience. The prominence of the volume of acquired habits in human coordination intimated that all basic coordination had originated in a similar way. By extrapolation from his own faculties, *man* conceded *experience* a role in the primary modelling of coordination, as well as a capacity to remodel such coordination once it had ceased to serve the needs of the body. In contrast to this view, we now learn of the great rigidity and unmodifiability of coordination patterns in the *lower vertebrates*.

Since it would be entirely unsatisfactory to let the matter rest with this obvious schism between lower and higher vertebrates, it seems desirable to indicate briefly how the gap can be bridged. In *amphibians* learning ability seems to be definitely confined to levels higher than S, with all partial acts below level S being firmly and irrevocably set. Thus an amphibian can be conditioned to exhibit a certain motor reaction, e.g., alarm, preying, retreat, etc., in response to a certain set of sensory stimuli. Yet, in producing these responses, it is bound to use the existing repertoire of preformed motor mechanisms, such as they are.

Proceeding to a higher animal, and choosing the *rat* as representative of lower mammals, the experiments of *Sperry* (1940, 1941) have demonstrated that the basic central scores are strikingly rigid and unmodifiable even on this scale of organization. The tendons of the major dorsi-flexor and plantar-flexor of the foot were crossed so as to cause either muscle to produce an excursion of the ankle joint in the opposite direction from normal. In order to avoid possible compensatory adjustments through normal

muscles, all muscles but the crossed ones were extirpated. After the operation, the rats first showed clear-cut reversal of foot movements, which was evident both in type reflexes and in complex postural and locomotor actions. If the central nervous system were to have re-integrated the ankle movements with those of the other joints, it would have had to retime the innervation of the crossed muscles so as to excite either of them at such moments when in normal coordination the other would have been activated. That such retiming would have led to mechanically and biologically satisfactory results, was shown by crossing the nerves to the crossed muscles. As we mentioned earlier, nerves transposed after their modulation has become immutable continue to operate according to the time schedule of their original muscles. Thus, a *plantar-flexor* muscle, provided with a firmly modulated *dorsi-flexor* nerve, acts in the *dorsi-flexor* phase of each movement (Sperry, 1941). Since in the animals with crossed tendons the effect of a former *plantar-flexor* is mechanically converted into *dorsi-flexion*, the nerve crossing rectifies the reversal produced by the tendon crossing, and movements are again correct. However, in no case did rats after simple tendon crossing (Sperry, 1940) or simple nerve crossing (Sperry, 1941) learn to adjust their foot movements. They persevered in operating the hind limb muscles according to the inherited coordination scores and failed to re-arrange the timing even under crucial training conditions. In conclusion, so far as plasticity of basic coordination patterns is concerned, the rat possesses none in its *hind* limbs, and, therefore, lines up in this respect with the amphibians.

A repetition of the experiments on the *fore* limb, however, has suggested that there may be a significant difference between fore limb and hind limb coordination (Sperry, 1942). Tendon crossing in the fore limb again led to reversal of all standard movements and postures, in the manner described for the hind limbs. However, some rats seem to have discovered an emergency solution, consisting of locking the extended elbow joint mechanically so that the stiff fore limb can be used as a brace for the support of the fore body. In this manner the animals can avert the caving in of the elbow joint which would otherwise ac-

company the supporting phase of all movements and postures because of the translocation of the extensor tendons to the flexor side of the joint. Even while this adjustment is in effect, the transposed muscles continue to contract in their original phases. The adjustment consists of an appropriate twisting of the whole arm by the shoulder muscles rather than of a corrective retiming of the arm muscles. A point to be stressed in this connection is that the locked position is assumed and maintained only for the one specific act for which it has been acquired, namely, the support of the body, while in all other performances the elbow is still moved in reverse. Moreover, frequent relapses occur even during the supporting phase.

In other words, the basic patterns of coordination have not been remodeled, extensor and flexor muscles have not traded their phases of innervation as they would have had to do if the mechanical reversal were to have been compensated for, and in this respect the experiments on the fore limb merely duplicate those of the hind limb. However, in addition and on top of the immutable inherited pattern, a trick performance has been established, the locking reaction, through which the old automatic and inadequate response can be temporarily superseded in a manner profitable to the body as a whole. This new performance is neither a permanent substitute for, nor is it in itself a revised edition of, the old pattern. The old stereotyped automatism continues in existence, only intermittently covered up by the action of another nervous apparatus more responsive to the needs of the body.

The adjustment of the fore limb behavior is of a very crude and primitive nature. However, an adjustment it is, nevertheless, and possibly the first faint trace of that capacity for learned coordination, which has reached such high degree in man. Pending proof to the contrary, one would feel inclined to ascribe this incipient adjustive capacity to the beginning evolutionary efflorescence of the motor cortex. Accordingly, the cortico-spinal system would have to be considered as the mediator in these adjustments (see *Tower, 1936*), and the lack of secondary adjustments of the *hind* limb movements of the rat could be correlated with the fact that in this animal only a small fraction of the pyramidal system

reaches the hind limb centers (*Ranson*, 1913). Through its short-cut from the cortex to the spinal efferent neurones, this system is obviously enabled to deal with the muscles directly under circumvention of the whole hierarchy of lower centers. Whereas such motor acts as are produced through the mediation of lower centers will continue to exhibit the stereotyped inherited patterns, responses effected over the cortico-spinal system may engage the muscles in new temporal groupings of varying combinations, to be deleted or retained depending on their ultimate success for the body.

Whether these new patterns are established by a trial-and-error procedure, or by virtue of some intrinsic self-regulatory capacity of the cortical system, is impossible to say and also wholly irrelevant from our present point of interest. The main thing is that this cortical activity, or to put it more cautiously, adjustive capacity of higher centers, is limited to setting up new secondary patterns without power to remodel or abolish the primary patterns. This would seem to imply that *the primary and secondary patterns are operated by different central mechanisms*.

As we go up in the scale of mammals, the wealth of secondary patterns—that is, of acquired performances, learned under the guidance of cortical activity—becomes so enormous that their preponderance tends to obscure the existence of the old primary patterns which dominated the amphibian picture. The presence of basic patterns of the primary unlearned type even in man, has of course been widely recognized. Studies on fetal behavior (*Hooker*, 1939) and child development (*Gesell*, 1929, *Shirley*, 1931) have been particularly suggestive. However, the distinction between primary and secondary patterns was usually based merely on differences of origin: autonomous central maturation of the former, as against peripheral acquisition of the latter by experience. There has been no intimation that the difference may also be one of plasticity. Hence, if we want to homologize the primary innate patterns of man with the basic coordination patterns of the lower vertebrates, we must first prove that they are equally unmodifiable. This is an empirical task which has not yet been accomplished thus far.

The problem is to separate those motor performances in which

the chronological scores of muscular contractions are absolutely fixed, and remain so even when they lead to unsatisfactory results for the body, from those in which the muscles can be operated in freely variable combinations so as to yield aimful responses. The most valuable experimental material bearing on the problem is in the hands of orthopedic surgeons, who are studying the recuperation of useful coordination after muscle transplantation in partially paralyzed limbs. Some well analyzed cases have brought to light a real conflict between inherited and unmodifiable patterns on the one hand, and novel patterns learned by experience with the aid of physical therapy, on the other (*Scherb*, 1938). However, no more than the first step towards a really clearcut classification and distinction in these matters has been undertaken.¹⁴

Another source of valuable evidence lies in the study of the comparatively rare cases of functional supernumerary appendages in man. One such case has been examined and has yielded some instructive data. A girl with three supernumerary fingers, which could be identified as a third, fourth, and fifth finger, when first tested, showed distinct "homologous response" between each extra finger and the corresponding normal finger of the same hand (*P. Weiss*, 1935b). Thus, obviously, the principle of myotypic response is as valid in man as it is in lower vertebrates. Whenever homologous muscles operate in association, this may be taken to indicate that they were activated through the mediation of the spinal mechanism to which the neurones of synonymous muscles respond in unison. However, after continued training with conscious effort, the girl finally managed to produce a very clumsy but, nevertheless, real dissociation between homologous muscles (*P. Weiss and Ruch*, 1936). She had apparently learned to innervate the extra fingers and the homologous normal fingers independently. This lasted, however, only so long as her attention was concentrated on the job. As soon as mental or physical

¹⁴ The author is at present conducting a research project jointly with the Orthopedics Department of the University of Chicago, in which oscillographic records of muscle potentials are used in the analysis of "functional restoration" after tendon transposition.

fatigue or distraction of attention weakened the effort, the old associated movements of homologous fingers re-appeared immediately. Here, too, cortical efforts have been successful in temporarily superseding and circumventing lower mechanisms with the result of greater refinement of movement, but this has not entailed a permanent reorganization of the lower effector patterns in the direction of better adjustment. These results, moreover, suggest that the adjustive higher mechanism (hypothetically identified here with the cortex) does not necessarily operate according to the myotypic principle, as the lower mechanisms of coordination do.

Of course, this is only a single case, and the examination leaves much to be desired. Much valuable evidence is constantly being wasted by not giving natural occurrences, such as the one here described, the critical experimental study which their fundamental significance would warrant. It is hoped that the concrete and differentiated questions raised in this paper may lead to increased interest in phenomena of this kind and to a more articulate evaluation of the information which they present.

CONCLUSIONS

It would be needless repetition to review here the specific conclusions reached from our experiments and deliberations, which have already been summarized in their context throughout this paper.

However, we may briefly examine how the answers to some of the standard questions about coordination will look in the light of our results. The term "coordination," one will remember, is used here strictly in reference to the fact that the central nervous system engages the muscles in such a definite order that, in a normal animal, their combined activities result in orderly movements, which, in turn, yield acts of biological adequacy for the animal as a whole.

It will have become obvious that questions such as: "*Is coordination inherited or acquired?*", "*Is coordination rigid or plastic?*", "*Is coordination under sensory control?*" just cannot be answered in that generality. There are patterns of coordina-

tion, as we have seen, that are definitely inherited, of prefunctional and pre-experiential origin; there are others that are definitely "learned." The former are rigid in some regards, but show a certain latitude in other regards. The latter show greater plasticity, but even so within definite bounds. Sensory "control" is not vital for coordination; however, coordination may suffer from its absence, the degree varying from one class of animals to another. We have found it necessary to speak of different degrees of coordination; to distinguish levels of coordination; to separate coordination of muscles in moving a limb from coordination of limbs in moving the body, and the latter from coordination of body movements for the satisfaction of biological needs. These are not all the same thing. They cannot be treated all alike and squeezed into a single formula. What holds for one level or one animal, cannot be applied as a matter of course to all levels and all animals.

Failure to recognize this truth cannot but breed sterile controversies. In fact, if it were not too far a digression from our main subject, it would be an easy matter to trace many a heated dispute of the past back to the fact that two schools, starting from two sets of different but equally valid data, generalized far beyond the legitimate scope of those data, ending up with irreconcilable theories, all in the name of simplicity. We prefer to think of natural principles as of great uniformity and universality, and we are partial to doctrines which present them as such. Accordingly, we would have expected coordination to be either all plastic or all rigid; all preformed or all individually acquired. To learn that it is partly one way and partly the other, is disappointing. Nevertheless, this is precisely what the facts have revealed, and we must acknowledge their testimony. Let us briefly review the evidence.

Is coordination inherited or acquired?

Undeniably, the *basic* patterns of coordination are inherited. That is, the nervous system of every vertebrate about which we have sufficient information, amphibian as well as mammal, develops a certain repertoire of patterns of coordination pre-

functionally. These patterns differentiate by virtue of the developmental dynamics of the growing organism in forward reference to their future function, but without the benefit of exercising that function during their formative period. They are laid down in a hierarchy of functional levels, of which the lowest, i.e., the one dealing directly with the muscles, operates in terms of specific signals, one signal for each individual muscle ("myotypic" principle). That the centers should be able to differentiate such a variety of specific signals (specific biochemical processes or specific electric states) is no more surprising than that different glands should be able to produce different secretions.

Once in operation, these basic patterns of coordination act "blindly," unconcerned of whether or not their peripheral effects are of service to the animal. Achievement counts in neither their making nor maintenance. In normal animals they are serviceable by predesign—evolution has taken care of that. When disarrangement of the bodily machine for which they are pre-adapted abolishes their serviceability, they continue unaltered. This, better than any indirect evidence, proves their preformed stereotypism. To this extent the data confirm the *preformistic* concept.

However, there is a second side to the story of coordination that is distinctly *non-preformistic*. The preformed patterns are relatively crude, and only grossly speaking are they stereotyped. The inherited repertoire provides an animal only with what we may call an existential minimum of vital performances. Improvements are called for and occur in varying degrees.

In this connection it should be pointed out that the inherited patterns, of course, do not arise all at the same time, nor all in the embryonic phase. Not only does the metamorphosis of amphibians furnish many dramatic examples of comprehensive behavioral changes during the functional life span, but a progressive expansion of the motor repertoire is plainly observable even in non-metamorphic animals. There is not the least doubt that this gradual enrichment has the same non-experiential origin as the earlier functional endowment, and is nothing but an external manifestation of the continuous progress in the elaboration of the central coordination systems by self-differentiation. The

inability of amphibians to readjust primary coordination at any phase in life seems to dispose of the possibility that coordination patterns first exhibited in later life may, in contrast to earlier ones, have been molded by experience.

The inherent repertoire of an amphibian is fully adequate to carry the individual through life without major changes, and, qualitatively, the animal must get along with its limited repertoire of "scores." However, there is room left for improvement on the quantitative side: in the readiness with which a certain score is activated, the smoothness and speed with which it is executed, and in its competitive rating relative to other scores. That is to say, the behavior of an amphibian can become "conditioned" to the exigencies of its environment by selective facilitation or inhibition of existing motor patterns. Within these narrow limits actual experience with the environment then modifies the structure of behavior—although not the structure of its component scores—, and within these limits the *heuristic* concept finds support. So much for the amphibians.

Vertebrates higher on the evolutionary scale, of more complex organization and more specific in their requirements, face more complex tasks. The elasticity of the inherited scores is becoming increasingly insufficient to meet the accidents of the environment. It is on this level of the scale that a new method of coordination came into being: coordination by individual design and discovery, rather than by predesign and evolutionary tradition. This new development culminates in man. Man can operate his muscles in ever varying combinations, can discover and retain successful effects, eliminate wasteful ones, and thus force his motor apparatus into increasingly better adapted service. Here non-preformed, "invented," coordination patterns become so prominent that they obscure the more ancient stereotyped patterns with which they coexist and overlap. We have tentatively identified this plastic coordination with the activity of the cortex, but there is no definite proof that subcortical functions may not take part in it in higher mammals.

In the lower mammals, this type of coordination is, if present at all, still in a very rudimentary condition. While both the

stock repertoire of the species and the ability of the individual to adapt the elastic stock performances to its needs seem to be considerably increased in the rat over what there is to be found in an amphibian, fundamentally the difference seems negligible as compared with the tremendous efflorescence of the ability to "invent" coordination patterns on the way from rat to man. On the other hand, the primitive trick adjustments of which the fore limbs of the rat are capable (p. 81) may be a true trace of emergent "inventive" coordination.

Whether this type of coordination attains its effects by trial-and-error procedure or by more direct means, as suggested by adherents of a systemic concept (e.g., *Goldstein*), is entirely beyond the competence of the present article to decide. The only positive statement we can make is that, in contrast to preformed coordination, the adequacy of the effects is the guiding principle in it, and that it employs different mechanisms from the ones through which the preformed coordination patterns are put into effect. As we have indicated above (p. 85), it does not, for instance, operate through the myotypic principle, and sensory control, Exner's "sensomotility," seems to play a much more constructive role than it does in preformed coordination. For the rest, adequate elucidation of these problems will come from the admirable progress of primate neuro-psychology.

The strictly preformistic concept must be qualified in still another respect. As it is usually presented, it is meant to imply preformation of function in form of a set neurone architecture: "anatomical" preformation. Because of this common connotation we have treated it above (p. 8) under the double heading of "preformistic-structural." But it will have become obvious from our investigations that such an implication does not have to be accepted. An account of some reasons why minute and systematic anatomical preformation of central functions must be questioned, was presented above in the chapters dealing with the myotypic principle. While it is needless to repeat the whole argument, we may restate the case.

The function of each motor cell is variable, depending on its modulation. The central coordination scores, on the other hand,

are invariable. Hence, their effect on the motor cells cannot be transmitted through fixed neurone connections. The existence of central states or processes of a high degree of specificity, a corollary of the myotypic principle, would explain the situation on a non-structural basis. In this view, "preformation" of a coordination pattern would mean that those hypothetic specific states or processes are activated in definite chronological sequences; much as in endocrine cycles, in which one gland activates another gland, which in turn, activates a third organ, and so forth, likewise without special anatomical channels to pipe the effect along. To fill the picture with concrete sense, is a task of the future. But we had to stress the point that to acknowledge the preformation of coordination does not mean to acquiesce in the strictly structural interpretation of coordination. Preformation of dynamics is the alternative.

Is coordination plastic or rigid?

After the foregoing remarks this question can be easily rectified. We must add: "Which type of coordination?" And, "Plasticity in what respect?"

If "plasticity" is understood in the sense of elasticity within a given qualitative performance, admitting of quantitative adaptation to what for the given species is a normal range of variability of the environment, both preformed and acquired coordination are "plastic." If, however, one defines plasticity as the ability of an organism to cope with emergency situations, lying beyond the normal range of elasticity, by creating new performances, previously not even latently in existence, the preformed type of coordination is definitely devoid of it. Since most of our experiments bear on this point, we need not labor it further.

Elasticity of performance is a different matter. As we mentioned before, there is latitude in the extent to which an animal draws on its motor repertoire, and in the rate at which it displays it. It may walk *or* swim, and do it either faster *or* more slowly. We also pointed out that the timing of the individual limbs in walking or swimming is not nearly as constant as the timing of the muscles in moving the limb (p. 36). But even so, the pattern

according to which the limbs are moved will always be the same for all four—following either the “progression” score or the “retreat” score—without a trace of dissociability and “plastic” re-coordination.

That nervous integration bears all signs of being a *self-regulatory systemic* action, such as we know them from other organic systems, is not to be doubted. As a matter of fact, one of the earlier “organismic” attempts to interpret “animal behavior as the reaction of a system” came from the present author (*P. Weiss*, 1925). To discuss the merits of this view, is beyond the scope of this paper. It is strictly within our province, however, to stress that if the systemic concept is to be reconciled with the facts as they now have come to light, the nervous system will have to be viewed not as if it were *one monotonous whole*, in which what happens to any one part has a direct impact on all other parts, but rather as *a system of systems, each of which consists of sub-systems*, and so on; in other words, as a *functional hierarchy* in which the competence, freedom of action, range of variability, etc., of each constituent member is strictly delimited by constitutional—and, on the higher levels, acquired—properties.

Thus, one might conceive, for instance, of the integrative complexes involved in progression or retreat as acting each as a single system, within which the actions of all component parts would be independent. However, the working units of these systems are in themselves systems of a lower order, in the present example elementary limb scores, and the integrative system is bound to operate through these subordinate systems as wholes. In other words, the systemic action on level S of the hierarchic scale (p. 24) might regulate the timing according to which the individual limbs are set in motion, but would be powerless to alter the limb scores themselves which operate on the lower level O. On this basis it would be understandable that after the amputation of a limb the timing of the remaining limbs can be aimfully revised for better service to the body (*Bethe*, 1931), while retiming of the muscles within the limbs is yet impossible in all lower vertebrates thus far studied, up to and including the rat. The assumption that the progression system and the retreat system are each

an entity, likewise explains the inability of our animals to piece together half of one with half of the other (p. 50). In a more general sense this view can be well reconciled, it seems, with the ideas of *Lashley* (1937, 1939) on systemic action of the brain, and with the known facts concerning vicarious action and compensatory regulation in integrative function.

However, we do not propose to go into the subject here any further. We merely wanted to indicate that the negation for lower vertebrates of "plasticity" in the sense of an omnipotent faculty to invent novel coordination patterns to meet emergencies outside of the elasticity limits of the inherited repertoire, and the affirmation of systemic properties of the centers, implying free interplay of forces within the limits of the constitution of the central system, are not at variance. There will be no misunderstanding on this point so long as one keeps in mind that the higher integrative functions are bound to operate through the existing lower functional mechanisms and that the latter, qualitatively determined during the developmental, i.e., pre-operational, phase, are inaccessible to reorganization. Only the higher mammals seem to have developed a new superstructure capable of setting up "plastic" coordination patterns by means which are not yet available on the lower levels of the animal scale.

SUMMARY

Experiments are described in which the method of transplantation of muscles and nerves was used to analyze the origin and, in certain regards, the operation of motor coordination in amphibians. The results support a preformistic concept of coordination in these forms. A basic repertoire of primary motor patterns develops during the developmental phase. These arise essentially by self-differentiation within the central nervous system, independent of the benefits of sensory control and guidance by experience. They are so predesigned that, when later projected into an anatomically normal peripheral effector system, they produce biologically adequate effects. If confronted, however, with an anatomically disarranged periphery, they produce correspondingly distorted effects without signs of corrective adjust-

ment. The relation of these facts to the phenomenon of "plastic" coordination observed in man and higher mammals, and their bearing on the theories of coordination is discussed.

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